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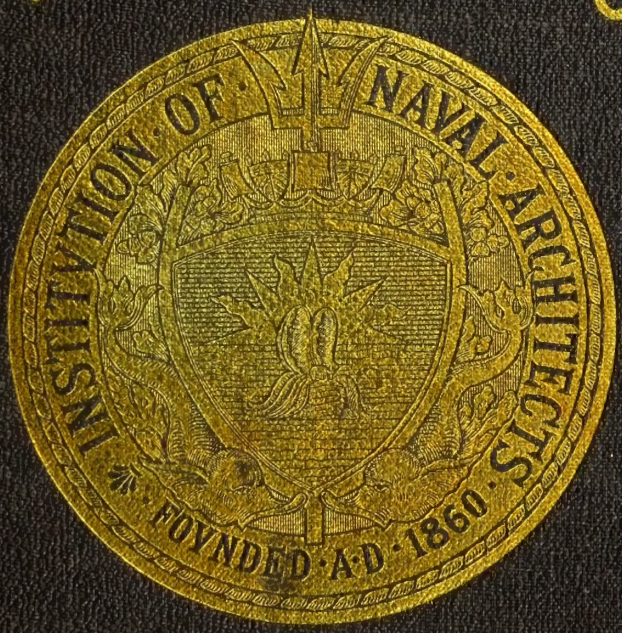

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OF
NAVAL ARCHITECTS.

1886

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VOLUME XXVII.

EDITED BY GEORGE HOLMES,

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1886.

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Mavor, Ivan.
McGregor, J.
McHardy, Wallace Bruce, Commander, R.N.
Medhurst, John T.
Milburn, J. D.
Milburn, William, jun.
Milne, Sir Alexander, Bart., G.C.B., Admiral of the Fleet (Vice President).
Milward, Thomas.
Moore, William W.
Moreaux, C. F. da S.
Moy, Thomas.

Neal, William George.
Neilson, Walter M., Colonel.
Newman, A. R.
Nicolson, Sir Frederick W. E., Bart., C.B., Admiral (Vice-President).
Noble, Captain Andrew, C.B., F.R.S.
Noel, G. H., Captain, R.N. (Assoc. Mem. Council).
Northbrook, The Right Hon. the Earl, G.C.S.I. (Vice-President).
Norton, Harold Percival.

Orlando, Paolo.

Pearse, Mountjoy.
Perkins, James.
Petke, Fritz.
Poretchkin, Theodore, Lieut., I.R.N.
Porter, Nicholas Ellis.
Price, Arthur, R.N.

Ramsden, Sir James, Kt.
Ravensworth, The Right Hon. the Earl of (President).
Rawson, Robert, Hon. Mem. Manchester Literary and Philosophical Society.
Rendel, Hamilton Owen.
Rennie, J. K.
Renwick, George.
Rice, J. M., United States Navy.
Riley, James.
Robinson, M. H.
Robinson, Sir R. Spencer, K.C.B., F.R.S., Admiral (Vice-President).
Rose, Captain J. B.
Rothe, H. P., Commodore, Danish Royal Navy.
Rowan, David.
Rowley, Charles J., Captain, R.N.
Rundle, Cubitt S.
Rothery, H. C., Q.C.

Samuelson, Sir Bernhard, Bart., F.R.S., M.P.
Sandham, Henry.
Sandison, Magnus.
Sosoa, Sachin.
Satrustegui, Don Patricia de.
Sandstrom, Theodore C. A., Com., Royal Swedish Navy.
Saunders, James Ebenezer.
Schmeman, Alexander.
Schüler, F.
Schunk, Theodore.
Scott, George J.
Serena, Arthur.
Sinnette, George Mathias.
Small, Thomas.
Smith, Charles William.
Smith, G. Fereday.
Smith, John.
Smith, Josiah T.
Smith, The Right Hon. W.H., M.P. (Vice-President).
Smyth-Pigott, J. H.

Stothert, G. K.
Stuart, Milton.
Stumore, Frederick.
Sudeley, The Right Hon. Lord.
Suffield, Thomas.
Swinburne, Thomas A., Captain, R.N.
Symonds, Thomas E., Captain, R.N.

Tatham, Hollings.
Taylor, J. M. H.
Temperley, Joseph.
Testa, Charles, Captain, Royal Portuguese Navy.
Thayer, Ignatius E.
Thomson, Andrew.
Thurston, Robert H.
Tindall, William H. (Vice-President).
Tough, R. T.
Tryon, George, C.B., Rear-Admiral, R.N.

Vavasseur, Josiah.
Vaz, J. M. P. da F., Captain, Royal Portuguese Navy.

Waddilove, Charles L. D., Vice-Admiral.
Watson, George.
Watson, Henry James.
Watson, William.
Webb, J. Beavor.
Westmacott, Percy G. B.
Wheatley, William Coe.
Wheeler, Joseph, jun.
Whyham, Maurice.
Williams, Samuel.
Williamson, R. H.
Wingfield, G. J., Lieut., R.N.
Wise, William Lloyd.
Woodall, J. W.
Woodward, Joseph J.
Woolfe, J. S.

Yeo, John, R.N.

Zeysing, Theodore, Prussian Royal Navy.
Ziffer, J. H.

OBJECTS OF THE INSTITUTION.

The objects of the Institution of Naval Architects—which was established to promote the Improvement of Ships, and of all that specially appertains to them—are comprised under three heads :—

First, the bringing together of those results of experience which so many shipbuilders, marine engineers, naval officers, yachtsmen, and others acquire, independently of each other, in various parts of the country, and which, though almost valueless when unconnected, doubtless tend much to improve our Navies when brought together in the printed Transactions of an Institution.

Secondly, the carrying out, by the collective agency of the Institution, of such experimental and other inquiries as may be deemed essential to the promotion of the science and art of shipbuilding, but are of too great magnitude for private persons to undertake individually.

Thirdly, the examination of new inventions, and the investigation of those professional questions which often arise, and were left undecided before the establishment of this Institution, because no public body to which professional reference could be made then existed.

BYE-LAWS AND REGULATIONS.

CONSTITUTION.

1. The Institution of Naval Architects shall consist of four classes, *viz.*, Members, Associates, Honorary Members, and Honorary Associates.

2. *Members.*—The Class of Members shall consist exclusively of Naval Architects, and Marine Engineers conversant with Naval Architecture.

3. *Associates.*—The Class of Associates shall consist of persons who are qualified, either by profession or occupation, or by scientific or other attainments, to discuss with Naval Architects the qualities of a ship, or the construction, manufacture, or arrangement of some part or parts of a ship or her equipment.

4. *Honorary Members.*—The Class of Honorary Members shall consist of persons who are eligible as Members, and upon whom the Council may see fit to confer an honorary distinction.

5. *Honorary Associates.*—The Class of Honorary Associates shall consist of persons who have contributed to the improvement of ships or their equipment, and upon whom the Council may see fit to confer an honorary distinction.

ELECTION AND DUTIES OF OFFICERS.

6. The Officers of the Institution shall consist of a President, Vice-Presidents, Members of Council, Associate Members of Council (not exceeding in number one-third the number of Members of Council), a Treasurer, two Auditors of Accounts, and a Secretary or Secretaries.

7. A General Meeting of the Members and Associates of the Institution shall be held annually before Easter in each year; and at this Annual General Meeting the Members of Council, Associate Members of Council, Treasurer, and Auditors for the ensuing year shall be elected.

8. At the annual General Meeting Members only shall vote in the Election of Members of Council, and both Associates and Members in the election of Associate Members of Council, the Treasurer and the Auditors.

9. As soon as may be convenient after the Annual General Meeting, the newly-erected Members and Associate Members of Council shall meet and elect the President and Vice-Presidents for the year, Members and Associate Members of Council both voting.

10. *President.*—Both Members and Associates of the Institution shall be eligible for election as President. The President shall preside over all meetings of the Institution, and of Officers of the Institution, at which he is present, and shall regulate and keep order in the proceedings.

11. *Vice-Presidents.*—Both Members and Associates of the Institution shall be elegible for election as Vice-Presidents. In the absence of the President, one of the Vice-Presidents shall preside at the General Meetings of the Institution, and shall regulate and keep order in the proceedings.

12. In case of the absence of the President and of all the Vice-Presidents, the Meeting may elect any Member of Council or Associate Member of Council, and in case of their absence any Member present to preside.

13. The Chairman at any Meeting of the Council of the Institution, when the votes of the Meeting, including his own, are equally divided, shall be entitled to give a casting vote.

14. Persons holding the office of Vice-President shall at all times be entitled to sit and vote with the Council.

15. *Past Presidents and Vice-Presidents.*—All Members who have held the posts of President and Vice-President shall, while their connection with the Institution as Members lasts, be entitled to sit and vote with the Members of Council.

16. *Members of Council.*—Members only shall be eligible for Election as Members of Council at the Annual General Meeting.

17. *Associate Members of Council.*—Associates only shall be eligible for Election as Associate Members of Council at the Annual General Meeting.

18. The Direction and Management of the Institution shall be vested in the Council for the time being, the Associate Members voting with the Members of Council in all cases, except in the decision of questions directly affecting the forms of ships and the construction of their hulls.

19. The Council shall meet as often as the business of the Institution requires, and at every Meeting five Members of the Council shall form a *quorum.*

20. The Council may appoint Committees to report to them upon special subjects.

21. All questions shall be decided in the Council by vote; but at the desire expressed in writing, of any four Members or Associate Members present, the determination of any subject shall be postponed to the succeeding Meeting of the Council.

22. An annual statement of the funds of the Institution, and of the receipts and payments of the past year, shall be made under the direction of the Council, and after having been verified and signed by the Auditors, shall be laid before the Annual General Meeting.

23. The Council shall draw up an Annual Report on the state of the Institution, which shall be read at the Annual General Meeting.

24. It shall be the duty of the Council to adopt every possible means of advancing the Institution, to provide for properly conducting its business in all cases of emergency, such as the death or resignation of Officers, and to arrange for the publication of the Papers read at the Meetings, or of such documents as may be calculated to advance the objects of the Institution.

25. *Treasurer.*—Only Bankers, or Members of Council, or persons who have been Members of Council and are still Members of the Institution, shall be eligible for election as Treasurer.

26. *Auditors.*—All Members and Associates of the Institution shall be eligible for election as Auditors.

27. The Auditors shall have access at all reasonable times to the Accounts of the pecuniary transactions of the Institution; and they shall examine and sign the annual statement of the Accounts before it is submitted by the Council to the Annual General Meeting.

28. *Secretary.*—The Secretary or Secretaries shall be elected by the Council, and shall be removable at the will of the Council, after due notice given. The salary of the Secretary or Secretaries shall be fixed by the Council.

29. It shall be the duty of the Secretary, under the direction of the Council, to conduct the correspondence of the Institution; to attend all Meetings of the Institution and of the Council; to take Minutes of the proceedings of such Meetings; to read the Minutes of the preceding Meeting; to announce donations made to the Institution; to superintend the publication of such Papers as the Council may direct; to have charge of the library, museum, and offices of the Institution; and to direct the collection of subscriptions and the preparation of accounts. He shall also engage, and be responsible for, all persons employed under him, and generally conduct the ordinary business of the Institution.

30. In January of each year the Council shall meet and prepare Lists for the election of the Council for the ensuing year. These Lists shall be as follows, namely :—

1*st.* A List of the names of the President, Vice-Presidents, and Treasurer for the ensuing year to be submitted at the Annual General Meeting, for their election in a body.

2*nd.* Lists for the election of the Ordinary Members and Associate Members of Council. This List shall contain the names of the existing Ordinary Members of Council and Associate Members of Council, and at least *six* new names of Members, and at least *two* of Associates of the Institution.

31. No addition shall be made to the total number of Ordinary Members of the Council until, by death or resignation, their numbers shall have been reduced below twenty-four, after which their numbers shall be raised to and preserved at twenty-four. And no addition shall be made to the Associate Members of Council until, by death or resignation, their numbers shall have been reduced below eight, after which their numbers shall be raised to and preserved at eight, always exclusive of the President, Vice-Presidents and Treasurer.

32. At the date of issuing the Syllabus of the Annual General Meetings in each year, the complete Lists proposed by the Council for the Ordinary Council for the ensuing year shall be printed, and sent to all Members to serve as Balloting Lists. These Lists shall contain, first, the names of the existing ordinary Members of Council at the time of the preparation of the Balloting List, together with six new names of Members of the Institution (or, if the number shall have been reduced below twenty-four, then so many new names as shall be needed to bring the number up to thirty), and the whole of these names shall be printed in alphabetical order. Secondly, the names of the existing Associate Members of Council at the time of the preparation of the Balloting List, together with two new names of Associates of the Institution (or, if the number shall have been reduced below eight, then so many new names as shall be needed to bring the number up to ten), and these names also shall be printed in alphabetical order. From these Lists the new Council shall be elected. Every Member shall be at liberty to strike out any name on the Lists, but he must strike out *six* names from the List of Members of Council, and *two* names from the List of Associate Members of Council.

33. A similar Balloting List (in which, however, the names of the ordinary Members of Council proposed for

Election shall not be included) shall be printed and sent to all Associates of the Institution, to serve as a Balloting List for Associates, from which the voting for Associate Members of Council shall be taken. Every Associate shall be at liberty to strike out any names on that List; but he *must* reduce the number by striking out *two* names from the List of Associate Members of Council.

34. The Balloting Lists may be sent by post or otherwise to the Secretary, so as to reach him before the day and hour named for the Annual General Meeting, or they may be personally presented by the Members and Associates at the opening of the Annual General Meeting.

35. At the opening of the Annual General Meeting the order of business shall be:—

(1.) To read and consider the Reports of the Council and Treasurer.

(2.) To read the List of Officers and Nomination for Council for the ensuing year, proposed by the Council.

(3.) The Chairman shall next put to the Meeting the List containing the names of the President, Vice-Presidents and Treasurer for election for the ensuing year.

(4.) The Chairman shall next nominate two Scrutineers (of whom one only shall be a Member of the existing or proposed Council), and shall hand to them the Ballot Boxes containing the Voting Papers for the ordinary Members of Council and Associate Members of Council; and

(5.) The Scrutineers, assisted by the Secretary and his clerks, shall receive the Ballot Papers which may have reached him, and all others which may be presented by Members or Associates at the Meeting. The Scrutineers shall then retire, and verify the Lists, and count the votes; and shall, not later than the following day, report to the Chairman the names which have obtained the greatest number of votes, subject to the conditions of the Ballot. The Chairman shall then read the List presented by the Scrutineers, and shall declare the gentlemen named in the List to be duly elected.

(6.) After the Ballot shall have been taken, and the Scrutineers have retired, the Meeting will proceed to the other business before it.

36. The new Council and Officers shall take office immediately after the close of the Annual General Meeting.

DESIGNATION OF MEMBERS AND ASSOCIATES.

37. Any Member, Associate, Honorary Member, or Honorary Associate, having occasion to designate himself as belonging to the Institution, shall state the class to which he belongs according to the following abbreviated forms, *viz.*, M.I.N.A.; Assoc. I.N.A.; Hon. Mem. I.N.A.; Hon. Assoc. I.N.A.

ELECTION OF MEMBERS AND ASSOCIATES.

38. *Admission of Members.*—Every Candidate for admission into the Class of Members, or for transfer into that Class from the Class of Associates, shall be more than twenty-five years of age, and shall comply with the following regulation:—

He shall submit to the Council a statement showing that he has been professionally engaged in shipbuilding or marine engineering for at least seven years in some public or private shipbuilding establishment, or marine engine works, and setting forth the grounds upon which he bases his claims to be considered a professional Naval Architect or Marine Engineer conversant with Naval Architecture, and to be admitted as such to the Membership of the Institution. This shall be signed by at least three Members whose signatures shall certify their personal knowledge of the Candidate, and approval of his statement;

or, in the case of persons not British born, the signatures of three Members shall be required, in confirmation of their personal knowledge of the Candidate's scientific reputation.

39. These preliminary conditions being satisfied, the Council shall then consider whether the practical experience and professional attainments of the Candidate are such as entitle him to be brought forward by the Council as a Naval Architect, or Marine Engineer conversant with Naval Architecture. If four-fifths at least of the received votes of the professional Members of the Council are in favour of his application, and such four-fifths constitute a majority of the professional Members of the Council, his proposal for admission shall be submitted to the Members of the Institution (who shall have access to the applicant's statement), at an Ordinary Meeting of the Institution, for them to vote upon, the voting to be by ballot, should a ballot be demanded.

40. *Admission of Associates.*—Candidates for Associateship shall submit to the Council a proposal for their admission, setting forth therein a statement of their claims to be admitted as Associates. Their proposal, if approved by the Council, shall be submitted by them at an Ordinary Meeting of the Institution, for the Members and Associates jointly to vote upon, the voting to be by ballot, should a ballot be demanded.

41. The proportion of votes for deciding the election of Members and Associates shall be at least four-fifths of the numbers recorded.

SUBSCRIPTIONS.

42. Each Member and Associate shall pay an Entrance Fee of two guineas, and an Annual Subscription of two guineas in advance; the first Subscription being payable on his election, and all future ones on the 1st day of January of each year. Any Member or Associate withdrawing from the Institution after that date is still liable for the amount of Subscription due on that day.

43. Any Member or Associate may compound for his Annual Subscription, for life, by a single payment of not less than thirty guineas.

44. No person's name shall be entered on the Roll as Member or Associate of the Institution nor possess the privileges of Membership (except it be on the honorary list) until he shall have paid his first subscription or the life composition, and if the payment be delayed for more than twelve months from the date of his election, the same shall be void unless the Council otherwise direct.

45. The Secretary shall at the close of every year notify to all Members and Associates whose subscription for that year shall not have been paid, that it will be his duty to report accordingly to the Council, and he shall at the same time furnish the person whose subscription is in arrear with copies of this and of the two following Rules.

46. The Secretary shall before Easter in every year lay before the Council a list of all Members and Associates whose subscriptions for the two previous years shall be still unpaid, and unless the Council shall otherwise direct, the names of those in arrear shall be expunged from the Roll of Members and Associates, and shall not be replaced without re-election in due form. Provided always that the Council shall at any time within two years therefrom have power to dispense with such re-election, and to restore the name to the Roll upon payment of all subscriptions then due, and upon cause being shown to the satisfaction of the Council why such subscriptions were not previously paid.

47. Nothing herein contained shall prejudice the right of the Institution to the legal recovering of all arrears of subscriptions up to the date of striking the name off the Roll.

48. In case the Council should be of opinion that any Member, who has been long distinguished in his professional career, from ill-health, advanced age, or other sufficient causes, should not be called upon to continue

his annual subscription, they may remit it. Also they may remit any arrears which are due from an individual, or may accept a collection of books, or drawings, or models, or other such contribution as, in their opinion, under the circumstances of the case, may entitle the person to be enrolled as a Life Subscriber, or to enable him to resume his former rank in the Institution which may have been in abeyance from any particular causes. These cases must be considered and reported upon by a Sub-Committee named for the purpose.

49. In case the expulsion of any individual shall be judged expedient by ten or more Members, and they think fit to draw up and sign a proposal requiring such expulsion, the same being delivered to the Secretary shall be by him laid before the Council for consideration. If the Council, after due inquiry, do not find reason to concur in the proposal, no entry thereof shall be made in any Minutes, nor shall any public discussion thereon be permitted; but if the Council do find good reason for the proposed expulsion they shall direct the Secretary to address a letter to the person proposed to be expelled, advising him to withdraw from the Institution. If that advice be followed, no entry on the Minutes nor any public discussion on the subject shall be permitted; but if that advice be not followed, nor a satisfactory explanation given, the Council shall call a Special General Meeting of Members and Associates, for the purpose of deciding on the question of expulsion; and if two-thirds of the persons present at such Special General Meeting, próviding the number so present be not less than thirty, vote that such individual be expelled, the Chairman of that Meeting shall declare such expulsion accordingly, and the Secretary shall communicate the same to the individual.

MEETINGS.

50. Meetings for the Reading of Papers shall be held as frequently, and at such times, as the Council may determine.

TRANSACTIONS.

51. The *Transactions* of the Institution, including the Papers read at the Ordinary Meetings, and Reports of the Discussions by which they are followed, shall be edited by the Secretary, and printed under the direction of the Council.

52. A copy of each Volume of *Transactions* shall be sent free to every Member and Associate.

53. The Secretary, under the direction of the Council, may dispose of the surplus stock of *Transactions* which have been published more than three years, at a price of not less than One Guinea a volume, provided a sufficient number remain on hand to supply the probable demand of new Members and Associates to complete their sets by the purchase of the back Volumes.

CHANGE OF ADDRESS.

54. Members and Associates are particularly requested to communicate to the Secretary any change of address.

INTRODUCTORY PROCEEDINGS.

THE Meetings of this, the Twenty-seventh Session of the Institution of Naval Architects, were held on the 14th, 15th, 16th and 17th April, 1886, in the Hall of the Society of Arts, John Street, Adelphi.

The opening Meeting was presided over by the Right Hon. Earl of RAVENSWORTH, President of the Institution, who commenced the proceedings by calling upon the Secretary, Mr. GEORGE HOLMES, to read the Report of the Council, which was as follows:—

REPORT OF COUNCIL, 1886.

"The Council is glad to be able to report that the financial condition of the Institution continues to be satisfactory. The statement of receipts and expenditure now submitted shows that the balance in hand has increased in spite of the large expenditure incurred under the head of Printing. The expenditure referred to was the last payment made under the old system of issuing a separate edition of the Papers before the publication of the *Transactions*. Thanks to the new arrangements announced in the last Annual Report, this item of expenditure has been very considerably reduced, so much so, that a saving of over £200 will appear in the next Annual Statement.

"The Council records with great regret the death of a most esteemed colleague, the late Mr. J. D'A. Samuda, who was for many years Vice-President of the Institution, and had been Treasurer since its foundation. He always took the deepest interest in the affairs of the Institution, and endeavoured to further its prosperity by every means in his power. A biographical notice, containing a sketch of his career and an account of his services, is published in the last volume of the *Transactions*. The Council endeavoured to show their respect for his memory by appointing a Deputation, consisting of the President and Sir Edward Reed, accompanied by the Secretary, to represent the Institution at his funeral.

"Acting under Bye-Law 24, the Council unanimously elected Mr. Henry Green, M.P., to fill the vacant office of Treasurer. Mr. Green has kindly accepted the nomination, and the accounts now appear for the first time with his signature.

"The new Regulations for the correction of the Reports of Discussions, announced last year, have worked so well that the Council was enabled to issue the last Volume of *Transactions* in two months after the Meetings. The Regulations will in consequence be continued in future.

"In December, 1883, the Council received an invitation from the Board of Trade to nominate a Member to serve on the Load Line Committee. As was announced in the Annual Report for the year 1884, the choice of the Council fell upon Professor Elgar, LL.D., who kindly consented to act, with the result expressed in the letter received from the Chairman of the Load Line Committee, which is appended to this Report. The Committee sat for about a year and a half, and after taking evidence in

London and at the principal outports, it finally reported to the President of the Board of Trade in August last. The Tables of Freeboard submitted with the Report received the sanction of the Board of Trade and of Lloyd's Register Society, and now form the standard by which the freeboard of ships is officially regulated. It is gratifying to know that the Committee's Tables appear also to have been favourably received by the shipowners throughout the country, and that an important step has at length been taken towards the settlement of the Load Line question.

"In conclusion, the Council congratulates the Members upon the large number of interesting and important Papers which are announced in the programme of the present Meetings, which are evidence of the great interest taken in the proceedings of the Institution, and a proof of its wide sphere of usefulness."

"LOAD LINE COMMITTEE.

"*August 10th*, 1885.

"*To the Secretary of the Institution of Naval Architects.*

"DEAR SIR,—The labours of the Load Line Committee having been brought to an end, I beg leave, as Chairman "thereof, to convey to the Council of the Institution of Naval Architects my very high appreciation of the services "rendered to the Committee by Professor Elgar, LL.D., who has represented the Council upon it. Professor Elgar has "taken a most active part in the deliberations of the Committee, and alike by his scientific and his practical acquaintance "with the conditions under which Merchant Ships are built and sailed, has greatly furthered its objects. In Sub-Committees "as well as upon the Main Committee he has contributed greatly to the work which had to be performed, and I venture to "think that it would not have been possible for the Council of the Institution of Naval Architects to have made a better "choice of a representative for assisting in the settlement of the important question of the Loading of Merchant Ships.

"I beg that you will convey to the Council my best thanks for the services rendered by their representative, and "I trust the Council may see fit to make to Professor Elgar a suitable communication on the subject.

"I am, DEAR SIR,

"Yours very truly,

"E. J. REED,

"*Chairman of the Load Line Committee.*"

Institution of Naval Architects.

Statement of Income and Expenditure for the Year 1885.

Dr.

1884.	£ s. d.	£ s. d.
Dec. 30th. To Balance :—		
In Treasurer's hands	754 12 2	
In Secretary's hands	40 6 10	
		794 19 0
1885.		
Dec. 31st. Annual Subscriptions		1,127 12 0
Sales of Volumes		157 19 9
Admiralty Grant		250 0 0
		£2,330 10 9

Cr.

1885.	£ s. d.	£ s. d.
Dec. 31st. By Cash paid :—		
Rent of Rooms		200 0 0
Cleaning Rooms		37 14 0
Attendance at Meetings		15 2 6
Reporting		50 0 0
Advertising		21 6 0
Printing Vol. XXV.		
Engraving Vol. XXV. ...	642 3 7	
Binding Vol. XXV.		
Miscellaneous Printing ...		
Despatch of Volumes ...	42 8 1	
		684 11 8
Stationery		13 4 6
Insurance		2 10 0
Bankers' Charges		1 0 3
Petty Disbursements		46 13 4
Furniture and Repairs ...		0 10 6
Gas at Meetings		3 3 0
Secretary's Salary	350 0 0	
Clerk's and Messenger's ditto ...	78 0 0	
Extra Attendance	7 8 3	
		435 8 3
Balance :—		
In Treasurer's hands	763 17 3	
In Secretary's hands		
Annual Subscriptions	14 14 0	
Sales of Volumes	2 0 0	
Current Expenses	38 15	
		819 6 9
		£2,330 10 9

LIBRARY FUND.

1884.	£ s. d.	£ s. d.
Dec. 30th. To Balance :—		
In Treasurer's hands		411 9 5
1885.		
Dec. 31st. Entrance Fees		77 14 0
		£489 3 5

1885.	£ s. d.	£ s. d.
Dec. 31st. By Library Expenses		10 18 9
Repairs of Furniture ...		14 3 0
Purchase of £402 0 2 Consols	399 9 11	
Commission ...	0 10 1	
		400 0 0
Balance :—		
In Treasurer's hands		64 1 8
		£489 3 5

HENRY GREEN, *Treasurer*,
GEORGE HOLMES, *Secretary*.

I have examined the above written entries with the books and vouchers, and find them correct.

JOHN CORRY, *Auditor*.

The following is a List of Donations to the Library for the year 1884:—

"Proceedings of the Royal Society." *Presented by the Royal Society.*

"Minutes of the Proceedings of the Institution of Civil Engineers," Vols. LXXIX., LXXX., LXXXI., and LXXXII. "Theory and Practice of Hydro-Mechanics, Session 1884-85, Charter, Bye-Laws, and List of Members," June, 1885. *Presented by the Institution of Civil Engineers.*

"Proceedings of the Institution of Mechanical Engineers," for 1885. *Presented by the Institution of Mechanical Engineers.*

"Journal of the Iron and Steel Institute," for 1885. *Presented by the Iron and Steel Institute.*

"Journal of the Society of Arts," for 1885. *Presented by the Society of Arts.*

"Transactions of the Institution of Engineers and Shipbuilders in Scotland," Vol. XXVIII. *Presented by the Institution of Engineers and Shipbuilders in Scotland.*

"Transactions of the Society of Engineers," for 1885. *Presented by the Society of Engineers.*

"Transactions of the North-East Coast Institution of Engineers and Shipbuilders, Vol. I., 1884–85. *Presented by the North-East Coast Institution.*

"Annual Report of the Board of Regents of the Smithsonian Institution," for 1883. *Presented by the Board of Regents.*

"Lloyd's Register of British and Foreign Shipping," for 1885-86. *Presented by the Committee of Lloyd's Register.*

"Lloyd's Register of Yachts," for 1885. *Presented by the Committee of Lloyd's Register.*

"Annual Reports of the Royal National Lifeboat Institution." *Presented by the Royal Lifeboat Institution.*

"The Scientific Proceedings of the Royal Society of Dublin," Vol 5. *Presented by the Royal Society of Dublin.*

"North of England Institute of Mining and Mechanical Engineers," Vol. XXXIII. *Presented by the North of England Institute.*

"Engineer," for 1885. *Presented by the Proprietors.*

"Engineering," for 1885. *Presented by the Proprietors.*

"Iron," for 1885. *Presented by the Proprietors.*

"Iron and Coal Trades Review, for 1885." *Presented by the Proprietors.*

"Field," for 1885. *Presented by the Proprietors.*

"Army and Navy Gazette," for 1885. *Presented by the Proprietors.*

"Shipping Gazette," for 1885. *Presented by the Proprietors.*

"Steamship," for 1885. *Presented by the Proprietors.*

"Saturday Review," for 1885. *Presented by the Proprietors.*

"Marine Engineer," for 1885. *Presented by the Proprietors.*

"English Mechanic," for 1885. *Presented by the Proprietors.*

"Revue Maritime," for 1885. *Presented by the French Ministry of Marine.*

"Rivista Marittima," for 1885. *Presented by H. Clauson, Esq.*

"Proceedings of the Royal Society of Victoria," Vol. XX. *Presented by the Royal Society of Victoria.*

"Registro Italiano," for 1886. *Presented by Cavalier F. Fassello.*

"Germanischer Lloyd," for 1886. *Presented by* ALFRED A. DIETRICH, Esq.

"Annalen Fur Gewerbe and Bauwesen." *Presented by the Proprietor.*

"Wochenschrift des Osterreichischen Ingenieur-und Architekten-Vereines. *Presented by the Society.*

"Science," for 1885. *Presented by the Proprietors.*

"The American Engineer," for 1885. *Presented by the Proprietors.*

"The Journal of the Franklin Institute," for 1885. *Presented by the Institute.*

"Proceedings of the United States Naval Institute," for 1885. *Presented by the Institute.*

"Ordnance Notes," for 1885. *Presented from the Ordnance Office, War Department, Washington, D.C., United States.*

"From Keel to Truck," a Marine Dictionary, in English, French, and German. By Captain H. Paasch, K.C.A.R. *Presented by the Author.*

"Marine Propellers." by Sydney W. Barnaby, Esq. *Presented by the Author.*
"Modern Shipbuilding and the Men engaged in it." By David Pollock, Esq. *Presented by the Author.*
"Yacht Architecture." By Dixon Kemp, Esq. *Presented by the Author.*
"State of the Navy." By Henry F. Watt, Esq. *Presented by the Author.*
"Theorie des Schiffes." By Herr Victor Lutschaunig. *Presented by the Author.*
"Theorie du Navire." By Herr Victor Lutschaunig. *Presented by the Author.*
"Costruzione e Tecnologia Navale." By Herr Victor Lutschaunig. *Presented by the Author.*
"The Strength and Proportions of Riveted Joints." By Bindon B. Stoney, LL.D., F.R.S. *Presented by the Author.*

The following Gentlemen (having been duly recommended by the Council), were unanimously elected Members of the Institution :—Mr. George Alexander Agnew, Shipyard Manager, Messrs. R. Napier and Sons, Govan, Glasgow; Mr. William Andrews, Surveyor to Lloyd's Register, Stockton-on-Tees; Mr. Thomas Ashton, Surveyor, Lloyd's Register, Swansea; Mr. Joseph Birnie, Marine Superintendent, Eastern Telegraph Company, London; Mr. William Boyd, Managing Director, The Wallsend Slipway and Engineering Co., Newcastle-on-Tyne; Mr. John Brockat, Engineer Surveyor to Lloyd's Register, Newcastle-on-Tyne; Mr. John Brodie, Graving Dock and Engine Works, Rotherhithe; Mr. Charles Buchanan, Surveyor, Lloyd's Register, Barrow-in-Furness; Mr. Charles Edward Burney, Surveyor, Lloyd's Register, Glasgow; Mr. Robert Bushby, Shipwright and Engineer, Surveyor, Calcutta, India; Mr. James Caldwell, Partner in the Firm of Messrs. Muir & Caldwell, Scotia Engine Works, Glasgow; Mr. John Freeman Fergus Common, Naval Architect, Cardiff; Mr. Thomas Henry Cooke, Surveyor, Lloyd's Register, Newcastle-on-Tyne; Mr. Richard William Coomber, Surveyor, Lloyd's Register, Bristol; Mr. Charles John Copeland, Managing Partner in the Firm of Messrs. Westray, Copeland and Co., Barrow-in-Furness; Mr. George Butt Craig, Partner in the Firm of Messrs. Craig, Taylor & Co., Shipbuilders, Stockton-on-Tees; Mr. William Jeffers Craig, General Manager, Victoria Graving Dock Co., Victoria Docks, London; Mr. Archibald Denny, Partner in the Firm of Messrs. W. Denny and Brothers, Leven Shipyard, Dumbarton; Mr. John Dickinson, Proprietor and Manager, Palmer's Hill Engine and Boiler Works, North Quay, Sunderland; Mr. William Dickinson, Manager, Palmer's Hill Engine Works, Sunderland; Mons. Paul du Buit, Manager and Chief, Shipbuilding Department, Société des Usines Franco Russes, St. Petersburg; The Honorable George A. P. H. Duncan, Partner in the firm of Messrs. Maudslay, Sons and Field, Engineers, London; Mr. Clennell Fenwick, Manager, Victoria Dock Engine Works Co., Victoria Docks, London; Mr. Frank Clement Goodall, Naval Architect, Southampton; Mr. William Hamilton, Shipbuilder, Port Glasgow; Mr. John Hamilton Harvey, Chief Draughtsman and Yard Manager to Messrs. Hamilton & Co., Port Glasgow; Mr. George Lisle Hindmarsh, Surveyor, Lloyd's Register, Glasgow; Mr. Thomas J. List House, Surveyor, Lloyd's Register, Whitehaven; Mr. James McNeil, Surveyor, Lloyd's Register, Hull; Mr. George Robert Mares, Surveyor, Lloyd's Register, Newcastle-on-Tyne; Mr. James B. Marshall, Constructor, H.M. Dockyard, Portsmouth; Mr. John William Miles, Surveyor, Lloyd's Register, London; Mr. James Mollison, Surveyor, Lloyd's Register, Glasgow; Mr. Hugh Muir, Partner in the Firm of Messrs. Muir and Caldwell, Scotia Engine Works, Glasgow; Mr. Henry Murray, Partner in the Firm of Messrs. Murdoch and Murray, Shipbuilders, Port Glasgow; Mr. David Purves, Engineer Surveyor, Lloyd's Register, London; Mr. Duncan Darroch Ritchie, Surveyor, Lloyd's Register, Barrow-in-Furness; Mr. Thomas Smith Ritson, Shipbuilder, Maryport; Mr. Walter Edward Robson, Surveyor, Lloyd's Register, Glasgow; Mr. Henry Adolph Salvesen, Superintendent Engineer, Messrs. J. T. Salvesen & Co.,

Grangemouth; Mr. George Scott, Messrs. Donald Currie & Co., London; Mr. James Sibun, Surveyor, Lloyd's Register, Newcastle-on-Tyne; Mr. John Edward Stoddart, Surveyor, Lloyd's Register, West Hartlepool; Mr. Edward John Tierney, Surveyor, Lloyd's Register, London; Captain Giuseppe Valsecchi, Naval Architect, Royal Italian Navy, Spezia, Italy; Mr. Frank Workman, Managing Director in the firm of Messrs. Workman, Clark & Co. Belfast.

The following Gentlemen were elected Associates:—Mr. Thomas Frederick Auckland, Rear-Admiral the Hon. E. R. Fremantle, C.B., R.N.; Commander Hubert Henry Grenfell, R.N.; Captain Walter Ernest Hutchinson, Mr. Stephen Jarman, R.N.R.; Major Charles Jones, Commander George S. Mac Ilwaine, R.N.; Mr. Alexander Mc Donnell, Mr. Ivan Mavor, Mr. William Milburn, jun., Captain Andrew Noble, C.B., F.R.S.; Mr. Hamilton Owen Rendel, Captain John Barton Rose, Mr. Magnus Sandison, Mr. Josiah Vavasseur, and Mr. Percy Graham B. Westmacott.

The SECRETARY next read the following list of names nominated by the Council to serve as Ordinary and Associate Members of Council for the ensuing year:*—Mr. William J. Bone, Mr. Richard J. Butler, Mr. William Denny, Mr. Raylton-Dixon, Mr. James Dunn, Professor Francis Elgar, LL.D.; Mr. J. MacFarlane Gray, Mr. Henry Green, M.P.; Sir Edward J. Harland, Bart.; Mr. Robert H. Humphrys, Mr. John Inglis, junior; Mr. William John, Mr. Alexander C. Kirk, Mr. James Laing, Mr. Henry H. Laird, Mr. George C. Mackrow, Mr. Frank C. Marshall, Mr. Benjamin Martell, Mr. James T. Milton, Mr. Henry Morgan, Mr. William Parker, Mr. William Pearce, M.P.; Mr. John Price, Mr. George B. Rennie, Mr. John Scott, Mr. Richard Sennett, Mr. J. Rodger Thomson, Mr. John I. Thornycroft, Mr. Phillip Watts, Mr. Bernard Waymouth, Mr. Henry H. West, and Mr. William H. White, as Members of Council; and the Most Hon. the Marquis of Ailsa, Mr. John Corry, Professor J. H. Cotterill, F.R.S.; Mr. E. A. Cowper, Commander R. N. Custance, R.N.; Mr. Richard S. Donkin, M.P.; Mr. R. E. Froude, Mr. Dixon Kemp, Captain G. H. Noel, R.N.; and Sir James Ramsden, Kt., as Associate Members of Council.

The PRESIDENT (Earl of Ravensworth) next put to the Meeting the following list, containing the names of the President and Vice-Presidents proposed for the ensuing year, and which was unanimously adopted:—The Right Hon. the Earl of Ravensworth, President; H.R.H. Vice-Admiral the Duke of Edinburgh, K.G.; the Right Hon. the Earl of Northbrook, G.C.S.I.; Admiral the Right Hon. Lord John Hay, K.C.B.; Admiral the Right Hon. Sir John Dalrymple Hay, Bart., K.C.B., D.C.L., F.R.S.; Admiral the Right Hon. Sir Cooper Key, G.C.B. F.R.S.; the Right Hon. W. H. Smith, M.P.; Admiral the Hon. Arthur Duncombe, Sir W. G. Armstrong, Kt., C.B., D.C.L., F.R.S.; Sir Nathaniel Barnaby, K.C.B.; Sir Frederick J. Bramwell, F.R.S.; Sir Thomas Brassey, K.C.B., M.P.; Admiral Sir Alexander Milne, Bart., G.C.B.; Admiral Sir Frederick W. E. Nicolson, Bart., C.B.; Sir Edward J. Reed, K.C.B., F.R.S., M.P.; Admiral Sir Robert Spencer Robinson, K.C.B., F.R.S.; Vice-Admiral Thomas Brandreth, Mr. F. K. Barnes, Mr. Peter Denny, F.R.S.E.; Mr. Henry Green, M.P.; Mr. George W. Rendel; Mr. W. H. Tindall, Mr. William H. White, Mr. James Wright, C.B.; and Mr. Joseph Woolley, M.A., LL.D., as Vice-Presidents; Mr. Henry Green, M.P., Treasurer; and Mr. John Corry and E. A. Cowper, as Auditors.

The PRESIDENT then nominated Mr. Henry H. West and Mr. C. H. Wigram as Scrutineers to examine the voting papers.

* For List of Members of Council actually elected, *see* p. xxxviii.

The PRESIDENT then proceeded to deliver the following Opening Address:—

Gentlemen, in rising to address a few observations to you, and to inaugurate our Twenty-seventh Meeting, I hardly need, I think, assure you what pleasure it gives me to meet you all once more—I hope in good health. I may also, at the outset, congratulate you upon the nature of the Report which has been read to you, which I think I saw by unmistakable signs was satisfactory to all here assembled, in its general tone and tenor, and as showing the increase of the strength and the importance of this great Institution. There is only one passage in that Report to which I would for a moment advert before passing to other matters. We can hardly expect, in a large Society such as this, that we should assemble after a lapse of twelve months without finding some gap or gaps in our ranks, that we should not miss the presence of some well-known form in this Hall, and lose the sound of some revered voice, in our Council Chamber. That is our lot to-day, and when I speak of Mr. Samuda, whose loss we all deplore, I speak of a man who was an original Member of this Institution, who had acted in the responsible position of its Treasurer for many years, and who had applied to that duty all the ability and the business-like habits of which he was so largely possessed, and who had devoted to the best interests of the Institution, throughout that long period, all the ability and all the energy of which he was so great a master. I am sure you all join with me in deploring his loss. That is the only remark I wish to make upon the Report, and I am sure it will find a response in all your hearts. I feel it most deeply myself, having had the pleasure and advantage of his intimate acquaintance for many years.

Now, Gentlemen, I will pass to other matters, and I am sure that you will agree with me that it is not a very agreeable, or probably a very easy task, to address an Institution of this kind, many, if not the greater number of the Members of which are more or less connected with what I always regard as the greatest of our national industries, at the present time, because there can be no sort of doubt that depression weighs upon almost every national interest in the country for some reasons which I am not prepared to go into to-day—for some unavowed reasons, it seems to weigh like an incubus, and I am sorry to say that the present prospects hardly justify much expectation of its immediate removal. It is natural, of course, when all our industries are seriously affected, that the great industries represented in this room of shipowning and shipbuilding, and marine engineering, should partake of that depression, and I am afraid it is my duty to point out to you that that unmistakably has been the case during the past year. There is no doubt whatever, I think, in anybody's mind, that the very great production of tonnage in 1881, 1882 and 1883, which was concurrent with a decline in trade, has largely added to the depression of our shipping interest. It is not, Gentlemen, that we do not enjoy, fortunately for this country, as large a share of the world's trade as we ever did, we carry as large a proportion as we ever did, I believe, but it is the price, the rate at which produce has been carried, that is such a serious consideration—the miserably low freights, in fact, that have ruled now for a very long time; so low is it, that steamers, even under the very best management, have hardly been able to run at a profit, certainly the margin of profit was extremely narrow, and I am sorry to say that a very large number of steamers have undoubtedly been run at a loss, and the consequence has been great distress to the shareholders, and in a very large number of cases the compulsory laying up of those steamers. I find that in my own Port, with which I am connected, that so lately as the beginning of this month (and I take it the Tyne is a pretty good standard to go by, being a very large commercial Port, and I am afraid probably other commercial Ports will present a similar

spectacle), there were laid up in the Tyne one hundred and twenty-one steamers and twenty-two sailing ships, a total of one hundred and forty-three, with a registered tonnage of steam of 98,152 tons, and a sailing tonnage of 14,436, being a total tonnage laid up of 112,588 tons. That was on the 1st of this month, but I am happy to tell you that that shews a very considerable diminution compared with the amount of tonnage laid up at the beginning of the year, because the largest amount of tonnage laid up at the beginning of the year was within some small fraction of 150,000 tons of steam and sail together. I have spoken, Gentlemen, of the large production of tonnage which took place in the years 1881, 1882 and 1883, and I will just give you the aggregate production of tonnage during that time. In 1881 there were built 1,000,000 of tons; in 1882, 1,200,000 tons; and in 1883, 1,250,000 tons; and then we begin to see the turn of the tide, because in 1884 there were built only 750,000 tons, and in 1885, as far as I can gather, there were built only 540,420 tons. Those broad figures will pretty well shew the depression that has existed, at least in the shipbuilding, and of course necessarily in the marine engineering business of this country. I said that our share of the world's carrying trade shews very little if any diminution, and I think that the returns of the Suez Canal are quite sufficient to prove that statement, because, by the last returns that I saw, we were carrying 75 per cent., and rather more than 75 per cent. of the whole of the tonnage passing through that Canal. But, Gentlemen, as I said before, it is the price at which that produce is carried—the low rates of freight, that really constitutes the serious part of this question.

Now, Gentlemen, I will not trouble you with very close figures, but I have the items here which with your permission I will put in to confirm my statement. I will give you the general returns, if you will allow me, of shipbuilding for the last year, 1885. I find that the Clyde built two hundred and forty-one vessels, of 193,448 tons. That is a decrease compared with 1884 of 105,119 tons. The North-East Ports, I take them all together—I have the Tyne, the Wear, the Tees, and West Hartlepool—I will not trouble you with these small figures, but I daresay you will give me leave to put them in.* The North-East Ports together built two hundred and twenty-nine vessels of 233,352 tons, showing a decrease for those Ports compared with 1884, of 50,332 tons. But, Gentlemen, in the midst of this very dark prospect, I am happy to say that there is a considerable ray of light thrown by another return which does not attract so much attention sometimes as it deserves, but which I have looked at with some degree of satisfaction, and that is the return of the Registrar-General of Shipping. I find that there is a natural process of exhaustion going on, which, if it continues at its present rate, must materially lighten the stocks of extra tonnage in this country, and consequently produce a reaction in the right direction, and ensure, I hope, orders for our shipbuilders before very long. I find that, taking Colonial and British Registers together, in the month of January last there was a diminution of carrying tonnage of no less than 10,050 tons; that is of vessels removed from the Register, as compared with vessels registered, and in February the number of vessels removed from the Register amounted to no less than 20,621 tons, taking steam and sailing tonnage together. I have the details of this, showing exactly the number of iron steamers added to the Register, and of sailing vessels added to the Register, and of steamers removed and sailing vessels removed during the same time, which, with your permission, I will give to you.

Now, Gentlemen, I think you will agree with me that there is some little hope to be derived from a study of those figures. But there is another cause which is worth pointing out to you, because, although

* The detailed figures will be found on page xxxvi.

condolence under present circumstances is very useless, anybody that can throw a ray of light I think is bound to do so as a matter of encouragement. I remember very well that in the last serious depression of trade the first ray of light came from the West. We often in a fine glowing evening sunset, look out of our windows and see a bright brilliant glow, and we say it will be a fine day to-morrow. I think by looking to the West we shall see that ray of light, and I trust and hope that the presage may be fulfilled, and the ray of light comes from America. I understand that the American trade has shown a marked improvement within almost a very few weeks of the present time. But there is another very important matter which I should like to advert to. The Committee of the House of Representatives not long ago, by seven votes to six (a very narrow majority I must remind you) agreed to a Report which they had drawn up pronouncing in favour of the Bill authorising the free purchase of foreign-built ships for the foreign carrying trade of the United States. Now, that is a very important step, and it shows that protection has not fulfilled the expectations that were formed of it at one time among our friends in the United States, at any rate as far as regards their shipping interests, and I should think, in all probability, a considerable portion of what I may call at the present moment the surplus tonnage of this country, may reasonably be expected to find its way across the Atlantic. I felt bound to make those one or two statements in reference to the gloomy returns which it has been my painful duty to lay before you with regard to our own shipbuilding during the past year.

But, Gentlemen, there is one point with regard to the tonnage built in the last year which I feel bound, as you all take great interest I know in the question of steamers, to point out to you, and that is the increasing use of that invaluable material, steel, in the construction of ships. I find that out of a total tonnage built on the Clyde, of two hundred and forty-one vessels of an aggregate tonnage of 193,458 tons, no less than one hundred and six vessels with an aggregate tonnage of 92,866 tons were built of steel in the Clyde alone. The Clyde, we all know, has taken the lead for many years in steel; but I am happy to be able to shew you by my next figures that the North-East Ports have followed suit, and are largely engaged in building steel vessels at the present time. Out of a total number of vessels built in the North-East Ports, of two hundred and twenty-nine with an aggregate tonnage of 233,352 tons, fifty-nine vessels with an aggregate tonnage of 90,470 tons were built of steel. Now, I take other ports, Hull, Leith, Grangemouth, Dundee, Aberdeen, the Mersey, Barrow, Belfast, and Southampton—that is nine Ports—and I find that these nine Ports built one hundred and twenty-six vessels of an aggregate tonnage of 90,484 tons, of which fifty vessels with an aggregate tonnage of 39,952 tons were built of steel. There are very few Ports indeed which shew an increase of building over 1885; but I think I am bound to point out that Southampton does shew that increase. It built last year ten vessels with an aggregate tonnage of 19,193 tons, against an aggregate tonnage of 15,636 in 1884, that shews a marked increase; and as it is so rare an occurrence in the statistics of last year, I felt bound to point it out. There is another Port in which I have long taken a great interest, and I am sure you all join with me in that interest, and that is the thriving Port of Belfast. I find that during the last year out of seventeen vessels with an aggregate tonnage of 27,756 tons, no less than thirteen vessels with an aggregate tonnage of 20,560 were built of steel, and out of that number no less than twelve vessels with an aggregate tonnage of 20,492 tons were built by the eminent and, I am happy to think, thriving firm of Messrs. Harland & Wolff.

Now, Gentlemen, we will leave the dry department of statistics, and I will venture to ask your leave

to offer a few general remarks upon other matters. I have been obliged to paint a rather dark picture, but I am very far from wishing anybody to believe that last year was not full of interest—and full of interest to those two great industries which this Institution represents. Although trade has been stagnant, naval science has been extremely active. It almost appears, Gentlemen, as if bad times stimulate the activity, the intelligence, and the researches of our scientific men, and it is not very unnatural that that should be so, because bad times compel the shipowner to look around him, and by every conceivable means science, patience and research will furnish to diminish the cost of navigation, to save manual labour, and to save what is more important than anything the cost of fuel; and I should like to point out, with your kind permission, two or three directions in which the progress in Naval Science has been most noticeable during the past year. I will not confine myself absolutely to the limits of a year, but within a very recent period. Probably some of the matters to which I am about to refer will be familiar to you, but I daresay you will be glad to know that the unscientific world outside your ranks thinks of what you are doing, and pays its just tribute of respect to your energies and exertions. I have mentioned steel. We shall have a valuable Paper read on that subject. I have shewn you how largely its use has increased and is increasing in shipbuilding, and the only danger that I can see in regard to this invaluable material is that there may possibly be in some quarters an inclination to presume too much upon its unquestionably valuable qualities. There may be, possibly, a danger under the pressure of competition (and, remember, Gentlemen, competition is no longer confined to this country, because foreign competition has become a very stern reality, which we must look in the face) to rely too much upon the virtues of that metal, and to sacrifice something of strength for the sake of lightness, and we can only trust to the good sense of our shipowners and shipbuilders, and the valuable vigilance exercised by Lloyd's Surveyors to guard against any possible danger of that kind.

But before I pass from the question of steel there is one remark which I should like to be allowed to make. It has reference to the Cleveland steel which, of course, is a material of very great value to our North Country shipowners; and I put it rather in the shape of a question. I shall, no doubt, get an answer in the course of the Meeting, some time or other. With regard to the Cleveland steel which, as you know, is manufactured by the Basic process for the purpose of expelling the phosphorus, I understand—I shall be corrected if I am wrong—that Lloyd's tensile test ranges from 28 to 32 tons, whereas the Cleveland—the Basic steel—ranges from 24 to 27 tons; and suggestions were made, some time ago, to Lloyd's to make some modification in the severity of that test in favour of the Cleveland steel which, of course, from its vicinity, is a very favourite product in our North Country shipbuilding yards. Well, Gentlemen, Lloyd's, with that courtesy which they invariably exhibit, and that desire to meet anything like a reasonable demand, directed, I believe, a careful investigation into this subject by their Surveyors. I have not heard what the result of that investigation has been; but I thought, being interested in our North Country industry, I would just allude to it in passing, and I daresay I shall get some information on the subject in the course of the Meeting.

Now, Gentlemen, I come to what the Marine Engineer has been doing of late, and the progress exhibited in that direction is remarkable. It was my duty last year—at least, I felt it my duty—to show you by figures, of which the authority is undoubted, the increase in the use of the triple-expansion engine in our merchant ships. That increase goes on, and I may tell you that the number of sets that have been built at the present time amount to one hundred and forty-two; and that there are in course

of building one hundred sets of triple-expansion engines for the purposes of the Merchant Navy. I have got the figures here—I will not trouble you with the details, but I will ask leave to put those in as they are a proper sequel to the figures that I put in last year. But, Gentlemen, I find that the triple-expansion engine is now of general use in the Royal Navy, which is still more important—I won't say still more important, but quite equal in importance—taking it in its economical aspect and power combined. I find that in our three great types of war ships which, I am happy to observe, are in course of construction in our dockyards, that the triple-expansion engine is to be fitted to each one of these three types. Of course, the first type is represented by those two noble specimens of naval architecture and naval power—the *Nile* and the *Trafalgar*. Engines have already been contracted for, for both those vessels, and are upon the triple-expansion principle. Well then, I come to the next most valuable type of ship, of which I am happy to see that a considerable number are being built, both in our dockyards and by contract—I mean, the steel armour-plated cruisers of the *Aurora* and the *Immortalité* type. They are also fitted with triple-expansion engines. Then I come to the third type—a most remarkable and entirely novel type—which are known in the profession, I believe, as "torpedo-catcher" vessels, having an immense speed and extraordinary power for their size. They are to be fitted with the triple-expansion engines. I will give the figures of these vessels, because they are rather remarkable. I will describe them as the *Rattlesnake* and the *Grasshopper* class; the *Grasshopper* being built at Sheerness, and the *Rattlesnake* in the Mersey, by contract, by Messrs. Laird. The dimensions of those vessels are remarkable. Their displacement is 450 tons; their engine-power represents nothing less than 2,700 horses. That is six times their tonnage, and their estimated speed is to be 19 knots. I have now pointed out to you generally how this important engine has come into use quite recently in the Royal Navy by taking the three types of ships which are at present building. Now, there is another matter to which the energies of our Marine Engineers have been greatly directed, and more during the last year, I think, than any period within my recollection—and that is the all-important question of the economy of fuel; and it seems to me (and it is undoubtedly the opinion of many of our chief Engineers) that the forced draught is destined, before very long, entirely to supersede the natural draught; and it also appears that by a judicious combination of the forced draught with the closed ash-pit, with the transverse instead of the horizontal bars—a thick layer of fuel, and an accurate admission of the air from below the bars—that absolutely complete combustion can be reached, and has been reached; and the effect of that combination, as I understand it, is that the rate of combustion per square foot of grate per hour enables you to develope the same power of generating steam with a much smaller grate, and a smaller heating surface, and likewise with a smaller boiler. The great difficulty, and the only difficulty—only it is a great difficulty—seems to be precisely to determine, at the right moment, the right quantity of air to admit, in order to effect that complete combustion by means of the forced draught. That seems to be a subject admittedly of considerable difficulty, and a knowledge of it must be acquired by very careful experiments. But surely, Gentlemen, the experiments are worth the trouble, because by the saving of space, which I have endeavoured very imperfectly to describe (and you have very likely detected defects in the argument; but I believe, speaking generally, I have not inaccurately described the apparatus to which I am referring), you will be able to gain room available for cargo; and if you attain the same power with diminished size of furnace in that portion of the ship, the space so saved will be available for commercial and for freight-earning purposes, which is an important consideration at the present time.

Now, I will refer to one vessel. I daresay the attention of engineers—in fact, I know it, has been

called to the case of a screw steamer called the *Odiel*. She is owned by Messrs. Scott Brothers, of Newcastle, and her trial was witnessed, I believe, by Members of Lloyd's—I know it was by some of our chief engineers in the North, and by representatives of our great steamship companies from different parts of the country, and I believe it was allowed that those trials were thoroughly satisfactory, and I think I shall be able to show you, by giving you an extract from the engineer's log, that her first voyage was also eminently satisfactory. I have the report of the engineer here, and I will just read it to you, if you will allow me, because it shows this, which is really very interesting—I am referring now to the steamship *Odiel*, which is fitted with that apparatus which is known in the profession as the Ferrando apparatus; constructed, I believe, on the principle of the forced draught, absolutely closed ash-pit, and the increased power thereby produced. Her first voyage was to Huelva, and here is a passage from the engineer's log, "Machinery successful, speed $10\frac{1}{4}$ knots, consumption $11\frac{1}{2}$; had very bad weather. The *Odiel*, with the old arrangement—this is what I want you, Gentlemen, to attend to—never steamed over 9 knots, loaded on a consumption of about 12 tons, so you find there an increase in speed and a saving in fuel. She will be in the Tyne about the 16th, when all interested in these important improvements will have an opportunity of inspecting the machinery. I have alluded to that vessel, because it appears to me to be a very favourable illustration of the efforts of engineering in the direction of fuel economy, accompanied with at least equal, if not increased power. But there is another curious thing, that the fuel employed was what is called in the North "Ryhope Duff," which I may describe generally as a very inferior description of fuel, costing about half as much as the ordinary bunker coals. If that fuel can be used and those results attained, it shows a very important stride made in the direction I have been trying to make good, namely, of economy of fuel by the efforts of naval science.

Now there is another matter with regard to fuel upon which I would like to say a few words, and that is the substitution of oil for fuel. I know at the present time, as a commercial question, it is hardly probably worth discussing, but it happened to me last summer to meet with a very intelligent Russian officer, who told me some things in regard to the use of petroleum which are extremely interesting. He told me from his own knowledge that there was not a single steamer in the Caspian, or a single locomotive that he knew of, running upon the lines connecting the Caspian with the Russian territory to the North, that used anything except oil as fuel. I may be very credulous, but I am one of those who believe that oil is destined to play a very important part in the future. I believe that oil is a substitute of bountiful nature for what inevitably will occur, and that I am afraid at no very distant period, the great diminution in the output of our collieries in this country, because whatever doubts may be thrown upon oil, one thing is certain, that the supplies of it, the natural supplies, are almost inexhaustible. I daresay many gentlemen in this room have read a most interesting book—I am sure more interesting than any novel I ever read—written by Mr. Marvin, on what is called the Fire Districts in the neighbourhood of the Caspian. We know that deposits of this oil exist in the neighbourhood of Quetta, our own possession upon the South-East frontier of Persia, and we also know that quite recently deposits of oil (they do not yet appear to be very rich) have been found in the immediate vicinity of the Red Sea, no unimportant discovery either, and what is also a fact that cannot be disputed is that the development of the oil industry is one of the marvels of the age. It began quietly at first, but it has assumed a development that is really quite extraordinary, and the chief pioneers of the enterprise are the eminent firm of Messrs. Nöbel, at Baku. There is one thing I should like to say about oil. Experiments are going on, not on any considerable scale, but I know that oil apparatus was fitted to the *Himalaya*, and her voyage, which was

a trial, was described to have been satisfactory; and it was also fitted in a very small steamer, the screw steamer *Ryde*, which is now lying in the river, and no doubt can be seen by any person interested in this extremely interesting subject. But although it is claimed by the admirers of oil as a substitute for fuel to have enormous advantages on the ground of economy of space and cleanliness and so forth, and that, weight for weight, it has an efficiency of over 60 per cent. over coal, there is this awkward fact to be looked in the face, that it happens at this moment to be 100 per cent. dearer, and, therefore, as a commercial question, I very much doubt whether for some time it will come into general use But whenever the time comes when coal or the supply of coal is so much diminished (and we know as a fact that it is diminishing at a fearfully rapid rate in some districts) that it becomes a question of deeper sinking, attended with all its accompanying dangers and difficulties, then it is that the oil will become probably an extremely valuable substitute. But one thing I think I feel pretty well convinced of, that is its unfitness for war ships, and I earnestly trust it never will be adapted to such a purpose, because the idea of a shell lighting in an oil tank is only second in horror to the notion of the lodgement of a large charge of dynamite beneath the hull of a ship. We saw a terrible instance the other day reported from the Baku Roads of an oil ship, from its extraordinary inflammable nature, blowing up, and every soul being consigned to eternity at once. I cannot believe that it will ever be employed, because, after all, the cost of coal is an immaterial consideration in a war ship, but for the purposes of commerce there is very little doubt that at some not very distant date the oil industry will assume a very important place. But it is as to the oil carrying trade, Gentlemen, that I wish to make an observation. It has always been a matter of astonishment to me that our shipowners, with all their known energy, did not enter at an earlier period into this very important trade, because it has assumed a development, as I said before, which is second to no industry in the world within recent times, and what do I find as a fact? That during the late depression, when our shipbuilding yards are idle, and thousands of our hands out of employ, the Swedish shipyards are very actively employed in building something like seventy steamers, or more, for this particular and special trade.

There seems to be great difficulty in the construction of the right sort of tank steamer for this carrying trade, but I am very happy to see within quite a recent period, in the Tyne, at least—I do not know about other places, and I only speak from my own knowledge of the Tyne—ships are being built for this trade, and at this moment, in the Low Walker Yard, there is a vessel of so remarkable a type that I venture to say it will stand alone in the annals of naval architecture. I have seen a rough sketch only of it—I have not seen the regular sketch—but the machinery is placed entirely aft, the whole of the hull is intersected and divided into an infinite number of compartments, but the extraordinary ingenuity, as it strikes me, speaking as an unscientific person, is in the arrangements made for the peculiar properties of this extraordinary product, and its expansion and contraction under different conditions of the atmosphere. There are contrivances to meet that difficulty, and as a specimen of naval architecture, I venture to think that this vessel will create immense interest when she is completed. I do not know how near completion she may be, but I allude to it because it only shows, and I am now trying to confirm my position, that during the last year naval architecture and marine engineering have not been idle, and I quote this as an illustration of that circumstance.

Now there is another most important advance in science of which I had the privilege of reading something lately, and that is the employment of hydraulic power for machinery. It is valuable on many

accounts, but it is mainly valuable because of its movability, from its almost general applicability, and from its saving of the wear and tear of steam machinery. You find it useful in the manufactory, you find it useful on the quays and in the docks, and you find it equally useful on board ship, as we know, because of the facility with which those gigantic weapons of war, our guns, are moved, by means of hydraulic machinery; but what is very remarkable (and I have had the privilege of reading one of the most interesting reports I ever read in my life, furnished to the Committee of Lloyd's Register, about two and a half or three years ago, by Mr. Parker), is that in the great dockyard of Toulon everything almost is done by means of hydraulic machinery. That is a very important matter, because we know very well the extraordinary ingenuity of our French friends in adopting new appliances, and we know the care with which they go into scientific subjects. It is employed at Toulon for riveting, bending, flanging, punching, besides cranes, winches and capstans, and almost every purpose for which it can be, and one remarkable circumstance about its use is that the French seem to have appreciated before we did the difficulties of manipulating steel; it avoids distressing that rather eccentric, sensitive, and highly irritable metal, which does not, like a good many Christians, like to be hammered, and the French have employed the hydraulic machinery in working steel for a considerable number of years. That is another circumstance worthy of observation, I think, in recent times.

But there is another subject, Gentlemen, to which I will for a moment call your attention, and it will come home to all your feelings. I never remember a period at which the great problem of saving life at sea has been prosecuted with more energy than during the last twelve months. Now the saving of life at sea, the safety of navigation, is the very *raison d'être* of this Institution; all the great improvements that our naval architects and marine engineers have introduced have had that one great foundation, that one great object in view, the saving of life at sea. During the last twelve months we have seen presented to us the Report of the Load Line Committee. You have heard to-day the important part taken by Members of this Institution in that great inquiry, presided over with so much diligence and ability by Sir Edward Reed, one of our most eminent Members, because a great portion of the Committee was composed of Members of your own Institution, and we owe them our thanks, and the commercial world at large owes them its thanks, and its most grateful thanks, for their labours. The most satisfactory feature of the Report is, I think, this, that it is not based upon any imaginary crotchet of what should be or what should not be the load line, but it is based upon practical experience and knowledge and observation; and we owe certainly to Lloyd's a very great debt of gratitude for laying the foundation of the rules which are adopted by that Committee, and laid down for the use of our commercial marine, and those circumstances will have a very material bearing upon its general acceptance, if ever the time should come when the Legislature should think fit to require a compulsory load line in all merchant vessels. But I find that already a great step has been taken in advance in this matter, because our shipbuilders are now in the habit, very generally, of inclining vessels in order to ascertain their stability and the curves of stability to which they are liable on inclination, and also in connection with various cargoes. Of course, a great deal of discretion must always attach to stevedores and captains in regard to the difference of the cargo, the time of year, and the distribution of the cargo, but still there are guides, sufficiently simple to be within the reach of almost any captain, laid down by that most important Load Line Commission, and I also find that the firm of one of our very distinguished Members, Mr. W. Denny, actually, with every ship they turn out, issue instructions to the owners in regard to the load line,

and therefore it appears to me that the Load Line Commission is already bearing fruit of a most valuable nature, and all tending to the great subject which I am touching upon now, the safety of navigation, and with it the saving of life at sea. There is another matter in regard to this great question—there is another Commission sitting at this moment to enquire into the general subject. I, of course, feel the deepest interest in that, as it did so happen that I was a member of the Duke of Somerset's Commission several years ago, and no general enquiry with regard to the question of loss of life at sea has been entered upon since the Report of that Commission, which I think was in 1877 or 1878. We shall all await, of course, with great interest the Report of the Commission; but there is one remark which with the greatest respect I would venture to make before I sit down with reference to that Commission. I have never ceased to regret a determination to which they came at the very outset of their enquiry, namely, to sit with closed doors. I think it was a great mistake, and I think it was a great mistake for these reasons: I am very much afraid, in fact I know, that that decision created in the minds of shipowners a very unfavourable impression at the time. Certain charges had been made—loosely made, and thrown rather broadcast at a profession, a business for which I, for one, have always entertained the highest respect—I believe it to be the greatest of our national industries—and those charges were undoubtedly felt to be directed much too universally against the whole body, and that unfavourable impression was unquestionably confirmed by the decision of the Committee to sit with closed doors. I am very much afraid also that when the Report comes out, which we shall all look to with great interest, the weight that that Report ought to carry will be diminished by the knowledge of the public that they have been sitting with closed doors. I am referring to the original impression, and I am referring likewise to the effect the Report may have upon the public mind; and I do think that a Commission of that nature which appeals to the feelings of every man and woman in this country ought to have been an open enquiry, and ought to have been divested of any appearance of one-sidedness, because I am afraid the impression prevails that the Board of Trade wanted to make out their case, and it is much more easy to convey bad impressions than it is to remove them. Therefore it is, I regret, and have regretted ever since, the determination of that Committee to sit with closed doors.

Now, Gentlemen, I have only one more remark to make. I am quite aware that I have intruded upon you for almost an unreasonable length of time, but I cannot sit down without referring to an event which has been painful to all of us, and which has occurred within a very short period, I mean the loss of that magnificent vessel, the *Oregon*. She was the pride of Bantry Bay, the pride of the merchant service of this country, and we all know the dreadful disaster that befel her. I am quite sure that I shall express the feelings of many when I express my own deep sympathy with the Cunard Company in their most serious loss. It would be, of course, pending the important enquiry which must take place into the loss, absurd to enter into any question of the causes, but no doubt there will be brought out matters of the deepest interest and importance to shipowners and shipbuilders alike, whenever the Report is issued. But, Gentlemen, I should like to be allowed to make this one observation, and that is to pay my own, and I am sure my fellow-countrymen's tribute of gratitude and admiration, for the conduct of the North German Lloyd's steamer, *Fulda*, and that little pilot boat, at the time when the whole of the crew, I believe, nine hundred in number, were upon the sea as it were, because the vessel was sinking, and the ready aid afforded by them to those unfortunate shipwrecked persons. Gentlemen, these timely acts of ready help, assistance and kindness, are really the things that bind nations together

far more than all the efforts of our most skilled diplomatists to form alliances, because they bind the hearts of nations, and the memory of the conduct of the captain and the crew of that North German Lloyd's steamer will long remain engraved upon British breasts, and will tend to confirm, if anything is needed to confirm, that union with our noble German and American allies which I am so happy to think exists. Nothing in the world tends more to bind nations together than ties of humanity and Christian feeling. I thank you, Gentlemen, for the kindness with which you have listened to these remarks. They have been rather, I think, a serious tax upon your attention, but you have listened to them with very great patience, and I thank you most heartily for your kindness.

REGISTRAR-GENERAL OF SHIPPING AND SEAMEN.—RETURN FOR JANUARY, 1886.

VESSELS REGISTERED.	Tons.	VESSELS REMOVED FROM REGISTER.	Tons.
Iron Steamers	13,674	Iron Steamers	14,408
Wood ditto	34	Wood ditto	52
Total	13,708	Total	14,460
		Difference	752
Iron Sailing Vessels	7,408	Iron Sailing Vessels	3,233
Wood ditto	1,137	Wood ditto	13,847
Total	8,545	Total	17,080
		Difference	8,535

THE SHIPPING REGISTER.—Large Decrease of British Vessels.—According to the official returns of the Registrar-General of Shipping and Seamen, there were *added* to the register 69 vessels last month, the total net register tonnage of which was 15,183 tons; whilst in the same period there were *removed* from the register 142 vessels, of a total net tonnage of 35,804 tons. In the month, therefore, the shipping of the United Kingdom and the Colonies *decreased* by20,621 tons. About one-fourth of the reduction was steam tonnage. Not fewer than 35 vessels were broken up in the month, including five small steamers. In the month of January the corresponding reduction was 10,050 tons.

SHIPBUILDING IN 1885.

NAME OF PORT.	NO. OF VESSELS.	TONNAGE.	DECREASE OR INCREASE COMPARED WITH 1884.
Clyde	241	193,458	105,119 (*decrease*)
Tyne	105	102,998	13,985 (*decrease*)
Wear	46	61,700	36,964 (*decrease*)
Tees	57	34,338	2,402 (*increase*)
West Hartlepool	18	33,030	2,067 (*increase*)
Blyth	3	1,215	3,852 (*decrease*)
North-East Ports	229	233,352	50,332 (*decrease*)

Steel Tonnage Built in 1885.

NAME OF PORT.	NO. OF VESSELS BUILT.	TOTAL TONNAGE.	NO. OF STEEL VESSELS BUILT.	TONNAGE OF STEEL VESSELS BUILT.	REMARKS.
Clyde	241	193,458	106	92,866	CLYDE SHIPBUILDING.
North-East Ports ...	229	233,352	59	90,470	Great Declension in Launches. — During March, only ten vessels, large and small, have been launched on the Clyde. The tonnage is 8,169 tons—the smallest amount in that month for very many years. In March, 1884, the tonnage launched was thrice that of the past month, and in the preceding year it was nearly four times that of this year. Four of the vessels launched last month are 100 tons or less—yachts, river boats, &c. The falling off is over 50,000 tons when compared with the launches of that year of briskness, 1883.
Hull	12	6,191	3	3,573	
Leith	26	3,627	3	1,780	
Grangemouth	...	1,500	6	1,485	
Dundee	13	7,357	8	6,251	
Aberdeen	7	7,399	1	710	
Mersey	40	27,596	15	4,135	Laid-up Shipping.
Barrow	4	4,058	1	1,458	The Harbour and Ferry Committee reported that the following ships were laid up in the Tyne on the 1st inst.:—121 steamers and 22 sailing ships—total, 143. Registered tonnage:—Steam, 98,152; sailing, 14,436—total, 112,588. Largest amount of tonnage laid up at the beginning of the year, 147,968 tons, steam and sail.
Belfast	17	27,756	13	20,560	
Southampton	10	19,193	...	...	

Total Tonnage Built in United Kingdom from 1881 to 1885.

1881	1,000,000
1882	1,200,000
1883	1,250,000
1884	750,000
1885	540,420

List of Triple Expansion Engines built and being built in the United Kingdom, exclusive of Engines for Government Ships.

PORT.	NO. OF SETS BUILT.	AVERAGE PRESSURE.	NO. OF SETS BUILDING.
		lbs.	
Glasgow	35	155	16
Hull	21	150	7
Greenock	5	...	10
Belfast	4	150	3
West Hartlepool	36	150	20
Sunderland	7	150	16
Liverpool	4	150	2
Leith	6	150	3
Tyne	24	...	23
	142		100

After the reading and discussion of Mr. Heck's Paper, the Scrutineers appointed to examine the Ballot Papers for the Election of the New Council presented the following Report:—

Society of Arts, John Street, Adelphi, London, *W.C.*,
14th April, 1886.

The Right Honorable the EARL OF RAVENSWORTH, *President:*

MY LORD,—We have to report that the following Gentlemen have been duly elected Members and Associate Members of Council of the Institution of Naval Architects:—

Denny, W.	Laing, J.	Rennie, G. B.
Dunn, J.	Laird, H. H.	Scott, J.
Elgar, F.	Marshall, F. C.	Sennett, R.
Gray, J. Mc F.	Martell, B.	Thomson, J. R.
Harland, E. J.	Milton, J.	Thornycroft, J. I.
Humphrys, R. H.	Morgan, H.	Watts, P.
Inglis, J., jun.	Parker, W.	Waymouth, B.
John, W.	Pearce, W.	West, H. H.
Kirk, A. C.	Price, J.	

Associate Members of Council elected as follows:—

Ailsa, Marquis of.	Custance, Commander.	Kemp, D.
Corry, J.	Donkin, R.	Noel, Captain.
Cotterill, J.	Froude, R.	

We have the honour to be,

Your Lordship's obedient Servants,

HENRY H. WEST.
C. H. WIGRAM.

Since the issue of Vol. XXVI. the following Gentlemen have been added to the lists of Members and Associates respectively:—Messrs. George A. Agnew, William Andrews, Thomas Ashton, Joseph Birnie, William Boyd, John Brockat, John Brodie, Charles Buchanan, Charles Edward Burney, Robert Bushby, James Caldwell, John Freeman F. Common, Thomas Henry Cooke, Richard William Coomber, Charles John Copeland, George Butt Craig, William Jeffers Craig, Archibald Denny, John Dickinson, William Dickinson, Paul du Buit, The Hon. George A. P. H. Duncan, Clennell Fenwick, Frank Clement Goodall, William Hamilton, John Hamilton Harvey, George Lisle Hindmarsh, Thomas James L. House, James McNeil, George Roberts Mares, James B. Marshall, John William Miles, James Mollision, Hugh Muir, Henry Murray, David Purves, Duncan Darroch Ritchie, Thomas Smith Ritson, Walter Edward Robson, Henry Adolph Salvesen, George Scott, James Sibun, John Edward Stoddart,

Edward J. Tierney, Captain Giuseppe Valsecchi and Frank Workman, as Members: and Messrs. Thomas Frederick Aukland, Rear-Admiral the Hon. E. R. Fremantle, C.B., Commander Hubert Henry Grenfell, R.N., Captain Walter Ernest Hutchinson, Stephen Jarman, R.N.R., Major Charles Jones, Commander George S. Mac Ilwaine, Alexander Mac Donnell, Ivan Mavor, William Milburn, junior, Captain Andrew Noble, C.B. F.R.S., Hamilton Owen Rendel, Captain John Barton Rose, Magnus Sandison, Josiah Vavasseur, and Percy G. B. Westmacott, Associates.

During the same period the Institution has sustained the loss of the following Members and Associates:—Thomas Chapman, F.R.S. F.S.A., Vice-President, Camille Audenet, Honorary Associate; J. A. Welch, Member; John L. Schock, Admiral Sir C. F. A. Shadwell, K.C.B., T. Westhorp, and George Wood, Associates.

ON THE SPEED TRIALS OF RECENT WAR SHIPS.

By W. H. White, Esq., Member of Council.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 14th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The purpose of this Paper is to place on record certain facts respecting the performances of some recent war-ships, and to illustrate the remarkable advances which have been made of late in the speeds attained by battle-ships, as well as in the design and construction of their propelling machinery.

From the time of the "Warrior" onward for twenty years, the measured mile speeds of the swiftest armoured ships varied from 14 to 15 knots; now there are a considerable number of battle-ships afloat having measured mile speeds of 16 to 17¼ knots, and still higher speeds are contemplated in the very large ships building by Italy, as well as in some armoured vessels of smaller dimensions building for European navies. Vessels of the cruiser classes recently completed have attained speeds of 18½ to 19 knots when fully laden; but in this Paper attention will be chiefly confined to the armoured classes.

In the Royal Navy, the new departure for battle-ships may be said to date from the period (1878) when the design of the "Colossus" and "Edinburgh" was prepared. It was then decided to adopt a form of ship differing greatly from any that had been previously used. As the result of a careful investigation, it was anticipated that without exceeding the length of 325 feet, which by common consent had been accepted for first-class ships, a steam performance would be secured equal in economy to that obtained on the "Warrior" with a length of 380 feet, when steaming at full speed.

In the consideration of this matter we were greatly assisted by the experimental researches of the late Mr. Froude; but the selection of the form finally adopted, and the estimate of the engine power required were entirely based upon our analysis of the steam trials of earlier ships. After the sheer draught had been prepared, the usual model experiments were made at Torquay, the results proving somewhat better than had been previously

estimated. No change was suggested in consequence of the model experiments, which amply confirmed the anticipation, that the form selected, besides being as easily driven as the "Warrior" up to 14½ knots, would also be very well adapted for much higher speeds.

At that period the French designers were giving great attention to the use of "forced draught" in the stokeholds of their war ships; and their action necessarily affected what was done here. After careful consideration, however, it was decided to hold in reserve for the new ships any gain in power and increase in speed obtainable with forced draught or assisted combustion. And this decision has had the result of giving to all the ships thus treated speeds exceeding by a knot, or a knot and a half per hour, the speeds with which they were publicly credited at the time of their design. It should be understood that this advantage was clearly foreseen by us when the vessels were designed, but experience with forced draught appliances was then almost entirely wanting in this country, and until the "Conqueror" trials had been completed, the best course of procedure could not be decided upon.

The form selected for the "Colossus" and "Edinburgh" was adopted also for the "Collingwood" and five other vessels of the "Admiral" class, before there was a possibility of making any speed trials by which to verify the estimate. It will be seen, therefore, that the experiment made was on a very large scale; and there were not wanting those who anticipated possible failure. The matter was placed beyond doubt by the trials of the "Edinburgh" made in September, 1883, and the trials which have followed with the "Colossus," "Collingwood," "Rodney" and "Howe," have been equally satisfactory, confirming and extending in a very remarkable manner the results obtained in the "Edinburgh."

It is unnecessary to give in full detail the particulars of all these trials and a selection has therefore been made, which fairly represents the performances, and illustrates certain matters of general interest.

Table 1 gives the principal dimensions and particulars of the "Warrior," "Bellerophon" and "Hercules," together with those of four recent ships: the draughts and displacements are those corresponding to an average on the steam-trials, which will be mentioned hereafter. The "Warrior," "Bellerophon," and "Hercules" have single screws, the others are twin screw vessels. The "Edinburgh," "Collingwood," and "Howe" are identical in under-water form. The "Howe" was tried at her designed load draught, but the other two were at light draught.

In Table 2 appear the principal particulars of the speed trials of the ships mentioned in Table 1 ; and in Table 3 appear particulars of their machinery and screws.

Without making any detailed comparison of their dimensions, it may be noted that the "Howe" had practically the same mean draught and wetted surface as the "Warrior," and is 55 feet shorter ; but is 10 feet broader and nearly 800 tons greater displacement. Yet she was, on the whole, as easily driven as the "Warrior" up to the maximum speed (14·35) which the "Warrior" attained : and she reached a maximum speed of 17 knots. The comparative performances of the two ships, so far as the expenditure of power is concerned, will be seen by reference to Fig. 1. (Plate I.) For speeds below 12 knots, the "Warrior" has a slight advantage : for speeds above 12 knots the "Howe" has a slight advantage, but in neither case is the difference worth noting.

In Figs. 2 and 3 the "Admiralty Co-efficients" of performance for the two vessels are represented in the ordinary manner by curves. Comment is unnecessary, except to remark on the obvious worthlessness of the "midship section co-efficient" as a test of the performances of dissimilar forms.

As the "Warrior" and "Howe" have practically the same area of wetted surface, there is no reason for constructing any curves representing the expenditure of power in proportion to wetted surface. I have purposely omitted any estimate of the "augmented surfaces" by Rankine's method.

This comparison of the "Warrior" and "Howe's" performance makes no pretence to completeness of analysis, nor is such analysis possible. So many features of difference, besides those in form and dimensions and proportions, affect the performance, that we cannot assign the exact value to any of them, nor must it be overlooked that the "Warrior's" trials were made nearly 25 years ago, when the care bestowed upon steam trials was not equal to that now taken to ensure accuracy. But the broad facts above stated remain, and they are sufficiently notable.

No progressive trials are available for the "Bellerophon" and "Hercules," their recorded performances for certain speeds are therefore indicated by "spots" on Figs. 1, 2 and 3, and they will be seen to be greatly inferior to that of the "Howe" at the same speeds, notwithstanding the greater displacement of the latter ship. It may be added that if the "Hercules" had performed as well as the "Howe" or "Collingwood," she would have exceeded 16 knots an hour, whereas her maximum measured mile speed with 8,500 H.P. was 14·7 knots

The "Howe," although 1,000 tons heavier than the "Hercules," can be driven at this speed with a development of 5,900 H.P.

Here, also, only the broadest comparison is attempted, and there is not the least intention to discredit the performances of the earlier ships.

The relative performances of the "Howe," "Collingwood," and "Edinburgh," deserve brief consideration. They were tried at different draughts and displacements, and in the case of the "Howe" there was a further important change in the diameter and blade area of the propellers. The "Collingwood" and "Edinburgh" were practically identical in their performances up to the maximum speed (16 knots) attained by the latter ship. The "Colossus" on her trial practically repeated the maximum performance of the "Edinburgh," but being a sister ship she was not tried progressively. The "Collingwood" had been designed to be a knot faster than the "Edinburgh," with natural draught; and forced draught fittings had been applied to her so that it was anticipated she would develop about 9,000 H.P. with her stokeholds under pressure. On the first trials with the screws set to a pitch of 18 ft. $10\frac{3}{4}$ in. the maximum power indicated was rather under 8,400 H.P., with eighty-nine revolutions and a speed of 16·6 knots. It was evident, however, that a change in the propellers was necessary if the full steam-producing power of the boilers was to be utilised by running the engines faster. As an experiment the pitch was reduced to 17 ft. 6 in., the revolutions increased to 95·57 and the indicated power to 9,573 H.P., but the speed was only increased to 16·84 knots.

It became obvious, therefore, that a radical change in the propellers of the "Howe" was required, if the much greater steam-producing power of the boilers in that vessel was to be utilised in adding to the speed of the ship. It was hoped, as the result of experience, that by running the engines at a higher speed, over 11,000 H.P. would be indicated on the "Howe," and from the "Collingwood's" trials it was evident that smaller screws must be employed. The change made is indicated in Table 3. The diameter was decreased from 18 to $15\frac{1}{2}$ feet; the blade area was diminished by about 18 per cent.; the pitch was slightly increased. With these propellers the engines of the "Howe" ran at 107 revolutions, and indicated nearly 12,000 H.P., driving the vessel at a speed of 17 knots. On reference to Figs. 2 and 3 it appears that the "Collingwood's" larger screws, as first tried, were rather superior in performance to those of the "Howe" for speeds of $13\frac{1}{2}$ to $16\frac{1}{2}$ knots; but at the higher speeds the "Howe" has the advantage, and this is true also for lower speeds than $13\frac{1}{2}$ knots. For all practical purposes, therefore, the change made in the "Howe" was a decided improvement.

These facts furnish another illustration of our want of exact knowledge of the conditions affecting the efficiency of screw propellers; but an attentive study of them cannot fail to be of service to future designing of quick running engines, especially for war ships. It is by no means meant that in the "Howe" the best possible combination of machinery and propellers has been attained within the limits of weight. The experiments just described, however, and others of a similar nature recently made have afforded much useful information on points previously obscure.

Every one who has had experience in the development of forced draught will recognise the representative nature of these trials. As a rule, hitherto the problem to be solved in ships thus fitted has been how best to utilise a steam-producing power which exceeds what was anticipated, on engines with fixed cylinder capacities. Changes in the admission of steam and alterations in the propellers have been the two directions in which experiment was possible. But it has not always proved an easy task to discover the best form and size of propellers which would permit the engines to run faster and use the steam, and at the same time be efficient for the propulsion of the vessel.

The particulars of the machinery and trials of the "Impérieuse" are given on Tables 2 and 3, and although they are not so complete as those for the other recent ships they present many points of interest. At the present time the "Impérieuse" is the fastest armoured ship afloat of which the trials have been completed, and a comparison between her performances and those of the "Bellerophon"—the ships being of approximately the same displacement—illustrates how far we have travelled in the direction of improved steaming performance in twenty years.

For the same indicated horse-power which was required to drive the "Bellerophon," 14 knots, the "Impérieuse" can be driven $15\frac{3}{4}$ knots.

The machinery of the "Impérieuse" and of her sister ship, the "Warspite," as well as that of the "Collingwood" may be regarded as belonging to the intermediate stage at which forced draught was treated as an *addition* to rather than as an essential element in the original design. The machinery of the "Howe," on the contrary, was from the first designed with a view to the use of forced draught, so far as experience up to that date permitted this course to be followed.

Table 3 contains some interesting data showing how the ratio of weight to I.H.P. on the measured mile trial, has altered from the time of the "Warrior" onward. It is unnecessary

for me to enter into any description of the differences in type of the engines as they are well known. But it is only right to remark that, since the time of the "Warrior," there has been a constant series of additions to the items of auxiliary machinery, pumping appliances, &c., included under the head of machinery in H.M. ships, so that the actual reduction in weight per I.H.P. is greater than would appear from the tables. Each ton of weight gave 6-horse power in the "Warrior," $6\frac{2}{3}$ in the "Bellerophon," $7\frac{3}{4}$ to 8 in "Impérieuse" and "Collingwood," and 10 in "Howe." I know of still greater developments of power in relation to weight as having been actually realised, and in many ships now building for the Royal Navy more sanguine expectations are likely to be fulfilled.

Many causes have contributed towards this reduction in the ratio of weight to horse-power. The most influential, no doubt, are the introduction of forced draught and quicker running engines. But it will not be overlooked that considerable savings in weight have resulted from the free use of wrought iron, forged or cast steel, and brass or gun metal, instead of cast iron in many portions of the engine framing, shafting, pistons, condensers, &c. This change of material has reduced weight without prejudice to efficiency and durability. The first cost, of course, has been made greater, but in a war ship, where reduction of weight is of the highest importance, this increase in first cost is not objectionable.

It is proper in this connection to mention an important distinction existing between the services of war ships and those of merchant ships, which distinction necessarily affects the design of the machinery, and, although well known, is sometimes overlooked.

Merchant ships are built and engined for the purpose of steaming continuously at certain maximum speeds. War ships, on the contrary, ordinarily cruise at very low speeds, and yet must possess power to attain high speeds when desired. The performances of the "Howe," contained in Table No. 2, will illustrate these remarks. At her full speed of 17 knots on the measured mile, this ship, with 107 revolutions, developed 11,600 I.H.P. Her engines, boilers and propellers had to be made such as would enable these conditions to be fulfilled. On ordinary service, the "Howe" would probably steam at from 9 to 10 knots an hour. Her engines make 51 revolutions, and develop 1,150 H.P. for 9 knots, the corresponding figures for $10\frac{1}{4}$ knots being 60 revolutions and 1,700 H.P. As a rule, therefore, the "Howe" would not require to use more than one-tenth of the full power which her boilers are capable of producing. It is clearly of the greatest importance that the power necessary to give her the high speed, at long intervals and for comparatively short periods, should

be secured with the least expenditure of weight consistent with assuring the maximum performance under special circumstances.

Another point worth mentioning is that these ratios of weight to I.H.P. are obtained under measured mile conditions, and not under circumstances of ordinary service at sea. No one is more alive to the difference between the two sets of circumstances than those who have to do with the design of war ships and their machinery. But the same course is followed for all the ships named in the tables, and indeed for the war ships of all countries, so that there is no impropriety or unfairness in the mode of comparison adopted.

One very interesting feature remains to be noticed, viz., the effect which recent changes have had upon the rate of coal consumption of war ships. Taking some of the latest ships tried with forced draught, and with moderate air pressure in the stokeholds, the following represents the results of careful observations made with return tube boilers :—

The maximum power obtained with forced draught and an air pressure not exceeding 2 inches of water has varied from 40 to 50 per cent. of increase above the maximum power obtained with natural draught. The rate of consumption of coal per I.H.P. with this type of boiler has been about 10 per cent. greater than the rate of consumption with natural draught and full power. The increase of power averaging about 45 per cent. has been obtained by burning about 60 per cent. more coal than with natural draught. With the steam blast as formerly fitted, about 20 per cent. increase on the natural draught power was obtained for an expenditure of 40 per cent. more coal. With boilers of the type fitted in the "Satellite," and described in Mr. Butler's Paper of 1883, the increase of consumption with forced draught would probably be somewhat greater. It appears to be within the truth to say, that with forced draught and full power the maximum rate of consumption of coal per I.H.P. per hour in the "Howe" does not exceed one-half the corresponding rate of consumption in the "Warrior."

On the basis of the measured mile performances, the "Warrior," with a coal stowage of 800 tons, could steam a little less than 3,000 knots at 10 knots in smooth water. The "Howe" starting with 800 tons could steam twice as far as the "Warrior" at that speed. At 14 knots the "Warrior" could cover about 1,100 knots, the "Howe" about 2,500. As a matter of fact, the coal bunker capacity in the "Howe" is 50 per cent. greater than that in the "Warrior," and the figures just given, therefore, have to be increased 50 per cent. in favour

of the "Howe's" "coal endurances." Speaking generally, the "Howe" can average about three times the distance which the "Warrior" could cover before her coal supply is exhausted. These facts are stated simply as indications of the great progress which has been made in coal endurance concurrently with increase in speed. The sources of economy in the modern ship are too well known to need comment.

It may be interesting to add in connection with the performances of these high speed twin screw ships, that a comparison of the results of model experiments with those of measured mile trials, shows the ratio of "effective" to I.H.P. to vary from 48 to 53 per cent. at the maximum speeds attained. The effective horse-power is taken on the *naked hull*, excluding the resistance due to shaft tubes, struts, &c. Without entering into any discussion of the relative efficiency of single or twin-screw propellers, it may be stated that these percentages show a high degree of efficiency in recent twin-screw ships.

I cannot conclude this Paper without recording my deep sense of the remarkably successful manner in which recent developments of marine engineering have been made available in the construction of recent war ships by my friend Mr. Wright, the engineer-in-chief, who has so long and so ably borne the responsibility for the steam machinery of the Royal Navy. Aided by his excellent staff, and assisted by the great private engineering firms of the country, he has enabled us, as a rule, hitherto to keep the lead in that essential quality, viz., the speed of our war ships. Private firms have also to be thanked for stimulating progress by the production of exceptionally swift ships, like the "Riachuelo" and the "Esmeralda;" and the wholesome rivalry between the Admiralty designers and private enterprise cannot fail to be an advantage to both.

TABLE I.

Particulars of Vessels.

	Length between perpendiculars.	Breadth extreme.	Mean Draught.		Area of Midship Section.	Displacement.	Wetted Surface.
	Feet.	Feet.	Feet.	Ins.	Square feet.	Tons.	Square feet.
"Warrior"	380	58	25	$11\frac{1}{2}$	1,219	8,852	30,200
"Hercules"	325	59	24	8	1,313	8,676	28,000
"Bellerophon"	300	56	24	2	1,200	7,319	25,560
"Edinburgh"	325	68	22	$9\frac{1}{2}$	1,287	7,750	28,000
"Collingwood'	325	68	23	9	1,354	8,200	28,700
"Howe"	325	68	26	8	1,557	9,637	31,000
"Impérieuse', ...	315	61	24	10	1,276	7,573	28,480

TABLE II.

Particulars of Steam Trials.

	Mean Draft. Ft.	Ins.	Displacement. Tons.	Indicated Horse Power.	Revolutions. Mean per Min.	Speed. Knots.	$\frac{M.S. \times V^3}{I.H.P.}$	$\frac{D^{\frac{2}{3}} \times V^3}{I.H.P.}$
"Warrior"	25	11½	8,852	5,469	54·25	14·356	659·4	231·5
	25	11½	8,852	2,867	44·5	12·174	767·1	269·3
	25	11½	8,852	1,988	38·0	11·04	824·9	289·6
"Hercules"	24	8	8,676	8,529	71·51	14·691	488·1	157·0
	24	8	8,676	4,045	55·29	12·123	578·3	186·0
"Bellerophon"	24	2	7,319	6,000	72·92	13·874	534·1	167·8
"Edinburgh"	22	9½	7,750	6,754	87·23	15·991	779·0	237·0
	22	8	7,690	2,537	62·33	11·961	862·0	262·8
	22	8½	7,710	1,508	51·19	9·849	811·6	247·3
	22	8½	7,710	815	41·05	8·097	834·4	254·2
"Collingwood" [The pitch of the propeller was reduced from 18 ft. 10¾ ins. to 17 ft. 6 ins. on the trial at the highest power, viz., 9573 I.H.P.]	23	6	8,080	9,573	95·57	16·844	666·4	201·0
	23	9	8,200	8,369	89·05	16·602	740·3	222·3
	23	10	8,240	7,071	85·47	16·051	795·3	238·6
	23	11	8,280	3,040	65·67	12·621	903·3	270·6
	23	11	8,280	1,597	52·18	10·236	917·2	274·8
	23	11	8,280	803	41·31	7·988	867·2	259·8
"Howe"	26	8½	9,658	11,613	107·24	16·923	651·0	189·3
	26	8	9,637	8,230	97·22	15·873	756·6	220·1
	26	8	9,637	4,099	79·15	13·386	911·1	265·0
	26	8	9,637	1,709	59·91	10·250	981·1	285·4
	26	8	9,637	1,150	50·93	8·938	966·7	281·2
"Impérieuse"	25	0	7,645	10,184	87·97	17·213	644·5	194·3
	24	10	7,573	1,567	47·89	10·096	837·9	253·2

TABLE III.

Particulars of Machinery and Screw Propellers.

	Indicated Horses Power.	Description of Engines.	Load on Safety Valves, lbs.	Number of Cylinders and their Diameters, Inches.	Length of stroke.	Piston speed.	Revolutions.	Propeller. In Twin-Screw Ships these particulars are given for one screw only.				Slip per cent.	Weight of Machinery.	Indicated Horses per ton weight of machinery.
								Diameter.	Pitch.	No. of blades.	Total Blade Surface.			
					ft. ins.	ft. per min.	mean per min.	ft. ins.	ft. ins.				Tons.	
"Warrior" .	5,469	Single screw Horizontal Trunk Jet Condenser.	22	2—104	4 0	435	54·25	24 6	30 0	2	116	10·62	884 (present weight, 1,016 tons.)	6·1
"Bellerophon"	6,000	Single screw Horizontal Trunk Surface Condenser.	30	2—104	4 0	583	72·9	23 6	20 1	2	105	4·01	895 (Present weight, 1,022 tons.)	6·6
"Collingwood"	9,573	Twin-screw Vertical Compound 3cylinders each set.	90	2—52 4—74	3 6	669	95·57	18 0⅞	17 6 (Originally set at 18.t. 10¾in.)	4	88·3	2·05 Neg	1,244	7·7
"Impérieuse"	10,184	do. do.	90	2—55 4—77	4 0	704	87·97	18 2	22 0¾	4	87·04	10·12	1,266	8·04
"Howe" .	11,613	do. do.	90	2—52 4—74	3 9	804	107·24	15 6	19 5	4	72·5	17·64	1,152	10·08

DISCUSSION.

Sir Nathaniel Barnaby: Gentlemen, it did not appear to me that the excellent Paper to which we have just listened gave much room for discussion, but it is rather a matter for congratulation, I think, that so great an advance has been made in speed and in economy of fuel. The Marine Engineers here will have remarked with great pleasure what advances have been made, and the ship builders will have seen how dependent they are on the Marine Engineer for the work which they have to do. Very much of the success which has been recorded by Mr. White so clearly and so fully has been gained by the closing in of the stoke-holds; very much has been gained by the large amount of attention which has been given to the subject by firms, several of which are represented here—English firms that have done their work splendidly, and if I might be permitted to say so, I feel myself deeply grateful to some of those who are here to-day for the work which they have done for the Royal Navy. When one comes to the question of the absolute value of the high speed in ships of that kind, it appears to be somewhat large and perplexing. I know it is the fashion to say that you should get the highest attainable speed in all kinds of ships. That is not my own opinion. I believe that the advances recorded by Mr. White, which are associated with a very large and a very valuable class of ship, are advances which are of the greatest possible value in smaller ships than those, but I do not believe that they possess so much value in the large ships as in the smaller ones. We have heard this morning how our friends, the Italians, have succeeded in getting a speed of 18 knots in their very large ships the "Italia" and the "Lepanto." I am very glad they have been successful, I think there are reasons why they should have gone in for a very high speed like that, and there would be, no doubt, on the part of the English Government, some justification for designing ships at least as fast as the 18 knots of the Italian ships, but that is not being done, and as I understand it, the naval people in England do not ask for it, in fact, I am betraying no secret when I say that when ships of 18 knots were commenced in Italy, the naval men in England said they would not know what to do with great ships of 18 knots speed, and when vessels of very high speed have been offered to them in England, as they have been to my certain knowledge for a good many years past, they have said that they prefer going in for lower speed and getting in those ships other elements which they regarded as being of higher value. In my opinion there is very much to be said for what I must hold to be, as far as I have gathered it, the naval view, that the first class heavily built and heavily armed ship need not have the highest very speed; that you want to associate with it smaller vessels with the highest speed you can possibly get. I think we may congratulate ourselves that the ships building in England at this moment are faster than ships of the same size building in foreign countries, although we have been told over and over again that it is not so. Mr. White's Paper this morning will disabuse your minds on that point, and will satisfy you that English built ships are really faster than the ships of the same size that have been built anywhere else. Whilst I am personally very glad, as you all are, that that is so, I must be understood to be doubtful as to whether too great a sacrifice may not have been made in attempting to secure the high speed in the most powerful class of vessels. If you had this for a policy, viz.:—we will not allow any Foreign Power to have a faster

ship of great power than we have in England, that policy I could understand, but that is not the English policy certainly at this moment, as we see by the latest ships that have been laid down, or if you had this policy, that you would insist on having a higher speed in our heavy ships of war than Mr. Thornycroft or Mr. Yarrow would put in the torpedo boats, that would accompany them or attack them, that I could understand, although I do not know how it could be done. That would be a thing well worth trying for. Either an attempt to get a higher speed than the torpedo boat has, or an attempt to provide for England higher speed in her first-class ships than any other power has, either of those would be intelligible policies, but I beg you to notice that that is not the English policy at this moment, and so far as I gather, it is not the naval opinion that it would be a right policy. As I gather the naval opinion, it is that there should be a balance of qualities in the heavy ship, that there should be a moderate speed, combined with the greatest possible offensive power, and as much defensive power as you can get; and together with those qualities combined in the large ships, you should take care and have a large number of ships costing very much less money, having the very highest attainable speed.

Admiral Sir JOHN HAY: My Lord Ravensworth and Gentlemen, first of all I would say that I am very grateful to Mr. White for this excellent Paper, and I would not have ventured to do more than thank him for it, were it not that I wished to disassociate myself so far as an old naval officer may, who has no particular right to speak on the subject, with what my friend, Sir Nathaniel Barnaby, said just now with regard to the naval belief, that it is undesirable to have great speed and superiority of speed in our large ships to those of any other nation. I would hope and trust that my gallant friend, Admiral Fremantle, whom to-day we have elected an associate of this institution, whose great knowledge of this matter, and whose youth will enable him to do good service in ships, fast or slow, as a young Admiral, will say a few words on this subject (I do not know whether any more of my gallant friends are here), as to whether he concurs in the opinion attributed to the Navy by Sir Nathaniel Barnaby on the subject of speed. It seems to me that if the Italian Navy or any other Navy have ships which exceed ours in speed, that if we are unfortunate enough to be at war with them we cannot catch them, I will not say we cannot get away from them, because that is a matter not to be considered, but if we are allies and our vessels are together acting in unison, as a chain cable is only as strong as its weakest link, so the fleet will only be as fast as its slowest ship. It is quite true, as we learnt last year on this occasion, I think from Captain Noel's most excellent Paper, that we have to balance the various advantages in a ship, yet at the same time I must say it is necessary, be the ships great or small, that speed should be considered. The direction in which naval architecture has advanced lately, has enabled naval officers to command fast ships of great size, and has been of great advantage to those who will command our fleets in war. We see, I think I may say, that the power of great speed which ingenuity has given us, not only gives great economy of fuel but power to proceed greater distances with the same fuel. All these points are of the greatest advantage to our fleets, but I must say, as a naval officer, I wish to express my own dissent from Sir Nathaniel Barnaby's statement, that it is the general opinion of the Navy (although, no doubt, there may be naval officers who hold that opinion and, no doubt, there are, or it would not have been stated by him)

that we should allow the great ships of foreign nations to exceed us in speed. I trust that the speed of the "Impérieuse," as indicated by Mr. White, may be exceeded, and the "Lepanto" may not discredit our naval architects by showing that the ships of Italy are superior to ours in one of the most necessary qualities that a man-of-war can possess. I may again be allowed to express a hope that my gallant friend, Admiral Fremantle, will not think that because he was only elected to-day, that the meeting would not like to hear from an officer of his great distinction and ability and knowledge of this particular subject, a few words with reference to the opinion of the navy. I do not know my gallant friend's opinion, he may be one of those who support Sir Nathaniel Barnaby, but I see him here, and I think whether he agrees with me or not, he will at any rate give us the great benefit of the information he possesses.

Rear-Admiral Fremantle: My Lord Ravensworth and Gentlemen, I certainly came here to-day quite as a stranger, through the kindness of my friend who read this Paper, and I did not know even that I was going to have the honour of being elected a member of this distinguished Institution, but I feel very grateful for that honour, and although I did not know it was going to be conferred upon me, yet I thankfully accept it. But, gentlemen, you can perfectly understand that when I heard my name read out I did not exactly know whether I was really elected, or that my name was mentioned for election at some future day. I state all this to you simply to explain that I had not the slightest intention of making any remarks on the present occasion, and it does seem to me to be somewhat presumptuous that I should do so. But the question that has been raised, and which my gallant friend Sir John Hay kindly stated that I would speak a few words upon, is certainly one that does interest me very much. I came here feeling the greatest interest in this Paper, especially as it was on the subject of the speed of large war ships. Now, I agree so far with Sir Nathaniel Barnaby, that if it is a question as to whether large ships or small ships should have great speed, if that were the question, if one depended at all on the other, I should say it is certainly of greater importance that a sacrifice should be made to give the smaller vessels large speed than it is that a great sacrifice should be made to give the large vessels large speed. The reason of this, I think, is obvious on the face of it. A small vessel must, under a great many circumstances, run away, and, therefore, necessarily must have heels to run away with, and also in our own Navy I am afraid it is the case that our smaller vessels have hitherto, until very recently been vessels which, in some curious way (for what reason I do not know) have been built with a small rate of speed. I do not blame the constructive department of the Admiralty, because we have just heard from Sir Nathaniel Barnaby that naval officers did not want high speed, but I think I am quite right in stating that, until some three years ago (I am not speaking by the card, but I am speaking from memory of a return to the House of Commons of ships recently built) all our small craft were designed for a maximum speed of something like eleven knots; if I am incorrect in that statement I shall be contradicted by those who are better acquainted with the subject than I am. But I must say that I entirely agree with Sir John Hay that speed in battle ships is very necessary, and I do not think I should be prepared to make any considerable sacrifice at all of high speed for the purpose of attaining other objects, desirable as they may be in themselves, and I will give one or

two reasons for that. There is a certain evolution which I am thinking of just now, and which I referred to in a recent lecture which I had the honour of giving at the United Service Institution, which I think would be an exceedingly good defence against a very good attack in a fleet action, the attack is one which was experimentally tried by the fleet under Sir Geoffrey Hornby in the past year. That evolution would only be possible with vessels on whose speed you could entirely depend. Were you not certain that your speed was at least equal to that of vessels of the other squadron that were attacking you, I do not think you would dare to make that evolution. I think, therefore, it will be found that in the evolutions of a fleet, it is absolutely necessary that speed should not only be considered, but should be one of the first qualities. Now, there is also the question of a reserve of speed, Let us suppose vessels with a nominal speed on the measured mile of fifteen knots, and if you want to drive the fleet twelve knots you will find some of those ships from reduced pressure in the boilers or from being out of trim, or for some reason, are not capable of keeping up that twelve knots. If the maximum speed of those ships is sixteen knots, the probability is that we should be able to keep that speed up. There is also the question of steaming through heavy weather—through the Straits of Gibraltar. I have been recently for three years stationed there, and I know what the levanter is blowing through the Straits, and also a westerly gale blowing through the Straits; it might be of the greatest importance to have a very high maximum speed in the battle ships of our Navy, to enable them to force through the Straits under those circumstances. Then there is the question with regard to torpedo boats under fire. It has been pointed out that with a limited number of torpedo boats coming to attack a battle ship, the best thing to do (if they are foolish enough to keep together, and if there are not many of them, they must keep together) is to turn your stern to them and give them your stern fire. Now, I calculated in the "Dreadnought," I think, with fourteen knots speed, with a torpedo boat steaming up astern of her at twenty knots, that you might have kept her six minutes and fifteen seconds under easy fire of our machine guns until she came within a proper distance to discharge her torpedo, and it is quite obvious if the speed were increased to fifteen or seventeen knots you would be able to keep her much longer under the fire of your machine guns. I do not think it is necessary for me to add anything further to those reasons which I have ventured at short notice to lay before you, as reasons why I think our big ships should not be deficient under any circumstances in speed; and if naval architects find that they can give us very high speed, and, I believe, they might, without any great sacrifice of other qualities, I think they are bound to do so. I would even go further, and say that I do not think it is unreasonable to sacrifice something even of carrying power, that it might be necessary to sacrifice something of protection and of offensive power to maintain high speed, I would only quote in confirmation of that the opinion of M. Gougeard, who is recently dead, the former Minister of Marine in France under Gambetta. He wrote a very valuable treatise a year ago on this question of torpedo boats, and the arm of the future, into which I will not enter now, but his conclusion is, that speed is everything. His view is, shortly put, that the torpedo boat will in process of time disappear as the fire ships have done, and it will disappear when the speed of the other ships is as great or greater than that of the torpedo boat. I do not entirely endorse that opinion myself, but I do think it is one which is very near the mark, and which has a very great bearing on the question of naval tactics, and on the whole question which has been brought

before us. My Lord, in conclusion, I would venture to thank the writer of the Paper for the extremely valuable remarks which he has made.

Admiral Sir Cooper Key: My Lord, I am sorry that I was not here when this Paper was read, or even to hear Sir Nathaniel Barnaby's remarks. I understood that the Paper was to be read at two o'clock. I mention this to show that it was my intention to have listened to this interesting Paper, which I have not read myself. I heard the concluding remarks of Sir Nathaniel Barnaby, and I think it is right that I should say that he and I were in accord on most points during the many years we worked together, and that during the last six years many ironclads have been designed and built, some of which are among the fastest ironclads afloat. It was our intention that the first class armourclads laid down during the periods mentioned should exceed the speed of those of any large maritime power by at least a knot. I need not say that Sir Nathaniel Barnaby carried out the views of the Board of Admiralty most successfully. The ships he has designed are quite beyond any praise on my part, he certainly worked with us and succeeded in providing ships of a very high speed beyond those of any other maritime power, except perhaps a few Italian vessels without side armour. I believe those Italian vessels are inferior to our later armourclads in defensive and manœuvring power, and therefore in qualities very necessary for battle. I am of opinion that to increase the speed of an ironclad more than we have of late, would necessarily be to sacrifice other important qualities. I have said sufficient to show that I consider speed is an important quality in an armour-clad ship. I will not trespass further upon your time.

Mr. W. H. White: My Lord and Gentlemen, the discussion has taken a turn which I never anticipated, and although strictly connected, no doubt, with some of the remarks in the Paper, I think scarcely a word has been said, as I hoped it would have been said by the engineers and shipbuilders present on the professional problems raised in the Paper. I can only hope and believe there is universal agreement with the opinions I have expressed. My desire has been to show that, thanks to the study of the problems of resistance and propulsion by the late Mr. Froude and his son, who is so ably carrying on his work, by our attempt to intelligently appreciate the principles of those investigations, and to give effect to them in the work we have to do, and by our independent analysis of the enormous mass of data which is to be found in the Book of Admiralty steam trials, we have been able to produce vessels which within certain limits of length, consistent with handiness, surpass in speed and in economy of propulsion anything that had been before accomplished. I do not say that at all boastingly. I simply put forward the facts. Working on a length that has been for twenty or thirty years accepted as the reasonable length for battle ships, results have been attained in the way of economy of power for speed which equal those attained in the days when 380 to 400 feet lengths were used. That success has been associated with a power of driving these recent vessels up to speeds that have hitherto never been approached by battle ships. The Italian ships which showed eighteen knots are about 14,000 tons displacement, and the "Impérieuse" is a little over 7,000, the "Howe," the largest ship in my list, less than 10,000 tons. Now it is not fair to speak of speed irrespective of size, and I have endeavoured to keep close to lines that will admit of a comparison. I

quite agree with the value of the reasons which Admiral Fremantle gave for having high speed and a reserve of speed in even the largest ships. Of course, every design of a ship is a balance of opposing claims, and it is very difficult sometimes to give a fair adjustment to those rival claims of speed, protection, armament, and power of manœuvring; but, speaking generally, I think we are in this position now—the gap between the torpedo boat and the battle ship is less than it used to be. Between the battle ship and the torpedo boat there was formerly a great gulf fixed in speed. We have now vessels of 1,500 tons steaming nineteen knots, and vessels of less than 4,000 tons steaming nineteen knots. We have the torpedo boat which is steaming, say twenty-two to twenty-three knots, while the "Italia" steaming eighteen knots is of 14,000 tons displacement, that is to say, there is no longer the great gulf between the ship of very large size and the torpedo boat. We have now every step in the series filled up to a speed approaching that which was formerly accepted for the torpedo boat and which sometimes was looked upon as unapproachable in ships of moderate size between the torpedo boat and the monster. It is the fact, that now we have in recorded trials the means of going to speeds in ships of not immoderate size, that only a few years ago were assumed to be impossible. The naval architect has greater assurance on that account than one would have dreamed of only ten years ago. I think, my lord, that is all I need say, except to thank the meeting again for the very kind way in which this Paper has been received.

The PRESIDENT: Gentlemen, I am sure, on your behalf, I may thank our friend, Mr. White, for his valuable Paper, and I may also, as President of this Institution, express the gratification which I am sure will be unanimously felt, that although he has risen to a high sphere since we last saw him, we are not going to lose the value of his assistance and instruction in our counsels in this hall. I beg to thank you, Mr. White, for the very valuable Paper which you have read.

AN IMPROVED MECHANICAL METHOD FOR FINDING THE STABILITY OF A VESSEL.

By J. H. Heck, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 14th, 1886; the Right Hon. the Earl of Ravensworth in the Chair.]

At the last meeting of this Institution, I had the honour of bringing under its notice a mechanical method for finding the stability of a vessel by the aid of a balance and inside sectional models.

Mr. Denny, of Dumbarton, during the course of last year, having courteously sent me two designs of an improved balance for use with inside models, led me to think out the plan by which outside sectional models could be used in place of inside models.

A sketch having been pencilled out, it was shown to Mr. A. Wilson (of Messrs. Hall, Russell & Co.), who kindly offered to make the apparatus, which I have at this meeting.

Experiments have been made by this machine on models of prismatic forms, and also on those of three vessels of very different types, and the results have been so accurate and so quickly obtained, that it was considered of sufficient value to form the subject of a Paper.

The chief improvements effected by the use of contracted outside models are as follows:—1st. The weight of the balance is greatly reduced, consequently the sensibility is increased. The balance which I have here weighs about 24 pounds, while that which I had here last year weighed 100 pounds.

2nd. The models are lighter and cheaper, as they can be made in about half the time that is taken up in the construction of inside models.

3rd. The practical difficulty of making the inside models perfectly watertight is entirely got over, and as the models can be taken out of the water after each inclination

and wiped before being balanced for the next angle, any error due to the absorption or adhesion of the water is eliminated.

By referring to the results of the experiments which are appended to this Paper, it will be seen that at none of the draughts or inclinations does the difference between the values of the righting lever got by the machine and those arrived at by calculation amount to one-tenth of a foot.

The models which were tried are here, and consist of about 12 pieces of yellow pine $\frac{3}{4}$ in. thick, fastened together by a few wood screws, the pieces being shaped to the respective form of the vessels, at regular intervals of from 16 to 30 feet, according to the length of the vessel, these intervals being obtained by dividing the length of the vessel into about 10 stations.

From Fig. 2 (Plate III.) it will also be seen that an appendage is left on three of the midship sections, in order that the model may be fastened to the table of the balance, and that the dimensions of a model for a vessel $258 \times 34 \times 23$ would be about $8\frac{1}{2} \times 6\frac{1}{2} \times 8$, and that its total weight does not exceed 7 lbs.

The balance used in the experiments, consisted of an oak frame A A Fig. 1 (Plate II.), attached to two steel bars B B, having knife edges.

At each end scale pans D D are suspended from knife edges, and at the centre of the frame is a table T fixed to a spindle working in bearings H H, which table can be turned and fixed at any angular interval of 15°.

In this machine the distance between all the steel bars is 12 inches, and the spindle of the table is exactly between the two steel fulcrum bars B B.

The oak frame works on the knife edge bars, upon two small metal plates K K, which rest upon two supports attached to a tank. This tank can be of any shape, as it is only required to hold water.

In order to determine the righting lever for any displacement or inclination, the model must be fixed to the table by two small bolts, as shown in Fig. 2, the balance being placed over the tank, so that the frame works upon No. 1 steel bar. Fig. 4 (Plate III.).

The table must then be turned to all the desired angles, for which the righting lever is required, and the frame balanced by putting weights into the No. 1 scale pan.

Next turn the frame end for end, so that it works upon No. 2 steel bar, Fig. 5 (Plate III.), and also find the weight necessary to be put into No. 2 scale pan, in order to keep the frame balanced at the various angles.

These weights can be noted down conveniently on a tabulated form, a copy of which, marked "Corrections," is given in the Appendix.

The following operation will now give the righting lever for any angle. (*See* Fig. 5).

1st. Turn the model to the required angle, and put the weight necessary to balance the machine into No. 2 scale. 2nd. Put water into the tank until a portion of the model is immersed, then the upward pressure of the water displaced by the model will force the model up and disturb the equilibrium. 3rd. Take weights out of the No. 2 scale pan until the machine is again balanced.

Then, evidently from the principle of the lever, weight taken out of scale pan $\times$ by its distance from fulcrum equals weight of water displaced by model $\times$ by the distance that the centre of gravity of the displaced water is from fulcrum.

Next turn the balance frame end for end, so that it again works upon No. 1 steel bar (*see* Fig. 4), and place the weight previously found necessary to balance the model when not in the water into the No. 1 scale pan. The machine will also in this case not now be balanced, because the upward pressure of the water has disturbed the equilibrium : if weights, therefore, are now taken out of the scale pan until the balance is restored evidently again,

Weight taken out of scale pan $\times$ by its distance from fulcrum equals water displaced by model $\times$ by the distance that the centre of gravity of the displaced water is from fulcrum.

The foregoing equalities will then determine the weight of the water displaced by the model, and also the righting lever for that inclination, for it is proved in the Appendix that the displacement of model equals the sum of the weights taken out of the scale pans, and distance of centre of buoyancy from spindle is equal to six times the difference of the weights taken out divided by the sum of the two weights.

Having this, the distance of the centre of gravity of the vessel, as marked upon the model, can be measured by a scale or rule and the righting lever consequently determined.

For greater or lesser displacements, it is only necessary to increase or diminish the height of the water in the tank.

Again a check test can, if desired, be soon made, for it is only necessary to replace the weights taken out of the scale pan and put weights into the scale pan at the other end of the balance, it will then be found that the weight required for equilibrium will be just half of that which had to be taken out.

This is quickly done and is of value not only because any error in noting or taking the weights can be seen at a glance, but also because any error in the machine itself, is practically neutralised by adding the two values together and dividing by two.

The foregoing may be briefly given as follows :—

In order to find the righting lever and displacement.

1st. Find the weights necessary to balance the model when not immersed in the water at all the required inclinations, both when the frame is working upon No. 1 steel bar and when working upon No. 2 steel bar.

2nd. Find the weight necessary to be taken out of the scale pan at the same angles in order to keep the machine balanced when the model is in the water, both when the balance frame is working upon the No. 1 steel bar and when working upon the No. 2 steel bar.

The displacement of vessel corresponding to each pound of water displaced by model can be found at once by calculation as the scale of the model is known ; or, if preferred, by the machine, as it is only necessary to divide the displacement of vessel at load draught by weight of water displaced by model at the corresponding draught in order to determine this.

The time required to make a model is about six hours, and all the data for complete cross-curves of stability, together with a check test, can be made in nine hours.

This method of finding mechanically cross-curves of stability, will some day be greatly simplified, as quite recently I made some experiments with a cardboard model and a small

balance only weighing a few ounces, which seems to promise a reduction, so far as time is concerned, if the cardboard or paper can only be made to stand immersion in water.

S. S. A.

Righting Levers.

Angles.	By Machine.	By Calculation.	Difference.	Displacement.
	Feet.	Feet.	Feet.	Tons.
15	1·22	1·275	·055	1660
15	1·54	1·6	·06	2150
15	1·51	1·55	·04	3920
30	2·4	2·33	·07	1663
30	2·81	2·74	·07	2408
30	2·725	2·8	·075	3354
45	3·31	3·36	·05	1200
45	3·27	3·3	·03	2220
45	3·65	3·67	·02	3820
60	4·11	4·11	·00	2286
60	4·57	4·52	·05	3414
60	5·06	5·07	·01	4363
75	5·316	5·3	·016	1770
75	5·6	5·64	·04	2773
75	5·92	5·95	·03	3800
90	6·63	6·55	·08	1791
90	6·814	6·83	·016	3300
90	6·898	6·86	·038	4140

Flush decked vessel.

Vessel 285 × 35 × 26 Feet.

Model made to a scale of one $\frac{1}{4}$ in. = one foot.

P. S. "B****."

Righting Lever.

Angle.	By Machine.	By Calculation.	Difference.	Displacement.
	Feet.	Feet.	Feet.	Tons.
30	2·654	2·7	·046	250
30	2·09	2·075	·015	360
30	1·018	1·025	·007	540
30	·45	·392	·058	640
60	1·654	1·625	·029	250
60	·656	·68	·024	414
60	·18	·125	·055	590
60	·36	·4	·04	650
90	1·01	1·1	·09	205
90	1·078	1·175	·097	286
90	1·191	1·275	·084	490
90	1·24	1·29	·05	570

Vessel 165 × 25 × 9·5 Feet.

Model made to a scale $\frac{1}{2}$ in. = one foot.

Fine light draught Steamer.

S. S. C.

Righting Lever.

Angles.	By Machine.	By Calculation.	Difference.	Displacement.
	Feet.	Feet.	Feet.	Tons.
15	·825	·875	·05	1903
15	·628	·7	·072	3004
15	·80	·85	·05	4350
30	1·8	1·725	·075	2450
30	1·57	1·485	·085	3630
30	1·159	1·15	·009	4490
45	2·829	2·82	·009	2092
45	2·22	2·3	·08	3150
45	1·372	1·44	·072	4060
60	2·997	2·95	·047	2090
60	1·99	1·94	·05	3390
60	1·33	1·32	·01	4184
75	2·175	2·22	·045	2200
75	1·536	1·525	·011	3474
75	1·146	1·09	·056	4220
90	1·28	1·27	·01	1427
90	·814	·8	·014	2942
90	·81	·74	·07	3910

Vessel 257 × 35·1 × 20·1 Feet.

Model made to a scale one $\frac{1}{4}$ in. = one foot.

Raised quarter deck vessel having bridge house.

APPENDIX.

DESCRIPTION OF THE STABILITY BALANCE EMPLOYED IN THE EXPERIMENTS (*see* FIG. 1.)

A A. Oak levers securely fastened to each other and to two steel bars B B., having knife edges.

C C. Steel bars at the ends of the levers, having knife edges, from which scale pans D D are suspended.

T. A small oak board (designated the table) attached to a steel spindle F, which works in brass bearings H H. This table can be set to any angular interval of 15°, and kept there by the bar M, which passes through the quadrant E, and the brackets N N.

E. A metal or oak quadrant fastened to the edge of the table, having slots cut out of it at angular intervals of 15°.

L L. Two oak supports fixed to the small tank, which is shown in the sketch.

K K. Small steel plates (resting on the support L L), upon which the balance frame A A works or oscillates.

The tank can be of any convenient form, as it is only required to hold water.

A small spirit level is fixed to the top of the levers A A, in order to ascertain when the frame is balanced. The top of the frame is marked, so that the distance between the fulcrum steel bars and the scale pan is divided into 10 equal divisions and each of the latter again divided into four parts. This is convenient, as it dispenses with the use of very small weights; as for example, if a ·1 lb. weight is placed on the No. 1 mark, it would really correspond to ·01 lbs. placed into the scale pan.

If to the No. 2, 3, 4, etc., it would correspond respectively to ·02 lb., ·03 lb., ·04 lb., etc., placed into the scale pan.

Again, if placed, say to the $2\frac{1}{4}$, $3\frac{1}{2}$, $4\frac{3}{4}$, $5\frac{1}{4}$, $7\frac{1}{2}$, it would correspond respectively to ·025 lb., ·035 lb., ·0745 lb., ·0525 lb., ·075 lb., in the scale pan.

TABULAR FORM.—CORRECTIONS.

	IN.	OUT.
Angles.	No. 1 Steel Bar.	No. 2 Steel Bar.
Upright.	11·535 lbs.	16·569 lbs.
15	12·73 „	15·435 „
30	12·9 „	15·12 „
45	13·7 „	16·275 „
60	14·345 „	14·95 „
75	15·195 „	13·13 „
90	15·87 „	12·355 „

The foregoing shows what weight (for a model) was required in the scale pan to balance the model when not immersed in the water. For example, at an inclination 60° when working on No. 1 steel bar 14·3 lbs. was necessary in the scale pan, together with ·1 lb. placed at the $4\frac{1}{2}$ mark, while when the balance was working upon the No. 2 steel bar 14·9 lbs. was required in the scale pan, together with ·1 lb. placed at the No. 5 mark.

Directions for making Outside Models.

Fig. 2 is a full size view of such a model for a vessel 258 × 34 × 23. As an illustration, suppose a model is required of the vessel (Fig. 3). 1st. Divide the longitudinal plan into ten divisions, giving in this case intervals of 24 ft., such as 1, 2, 3, etc., taking, if necessary, a half interval at the ends, as $\frac{1}{2}$ and $10\frac{1}{2}$, and then from the body plan make a tracing of the lines of the vessel at those sections.

2nd. Procure a plank of yellow pine which has been passed through the planing machine, having the thickness marked against the section in Figs. 2 and 3.

Note.—The model illustrated has sections $\frac{3}{4}$ in. thick; the variation at the ends is due to the employment of the half intervals.

Erections can be easily taken into account by making the thickness of the sections proportional to the length of the erections.

3rd. Cut the plank into lengths suitable for the various sections and make upon each piece a centre line A A and also at right angles the lines *x y* and *z z*.

4th. Lay the tracing of the body plan upon each section so that a horizontal line marked upon the tracing coincides with *x y*, and mark off the form of the vessel at that section.

5th. Take the section to a band saw and cut them closely to the pencil marks; at the same time drill the two holes H H. (*See* Fig. 2).

6th. Put the sections together and drive a long box or metal pin through the holes H H. This will keep the sections in line with each other. Next keep the sections close to each other by a carpenter's cramp and insert a few wood screws as shown.

The model can then be varnished, and, after the centre of gravity of the vessel is marked, is ready for use. Any convenient position for the centre of gravity will do, as a correction can at any time be easily made for any variation in its position.

Old models can be utilized for vessels of smaller dimensions.

If some planed planks be kept ready in stock, models can be quickly made.

By referring to Fig. 2 it will be seen that an appendage is left to the three midship sections in order that the model may be secured to the table.

Explanation of the principle on which the balance is based.

From Fig. 4 it will be seen that the distance of the spindle or axis of the table from the steel fulcrum bars is half that between the scale pan and the steel bars. As an example:

Let the value of the righting lever be required for an angle of 90°.

Suppose the frame is working upon No. 1 steel bar as in Fig. 4, put weights into No. 1 scale pan until the machine is balanced; turn the frame end for end as in Fig. 5 so that it works upon No. 2 scale pan and finds also the weight now required to keep the machine balanced.

Pour water into the tank to any height W L. Then it is evident that the upward pressure of the water displaced by the model will destroy the equilibrium, consequently weight must be taken out of the scale pan until the machine is balanced, then if D=water displaced by model, W=weight taken out of scale pan, x=distance from spindle of centre of gravity of water displaced by model,

$$12\ W = D\ (6 + x) \qquad (1)$$

In order to determine the displacement D turn the frame end for end again as in Fig. 4 so that it works upon No 1 steel bar, and put in the weight which was previously found necessary to balance the machine when the tank was empty of water.

The upward pressure of the water will again necessitate weights being taken out of the scale pan to keep the machine balanced.

Then if the weight taken out is called $\overline{W}$ we should also have

$$12\ \overline{W} = D\ (6 - x) \qquad (2)$$

By adding together Equations (1)+(2)

$$12\ (W + \overline{W}) = 12\ D$$

$$\therefore\ D = W + \overline{W} \qquad (3)$$

also by subtracting Equations (2) from (1)

$$12\ (W - \overline{W}) = 2\ D\ x$$

$$\therefore\ x = \frac{6(W - \overline{W})}{D} = \frac{6(W - \overline{W}}{W + \overline{W}} \qquad (4)$$

Equations 3 and 4 consequently give the displacement and the distance of the centre of buoyancy

from spindle for any angle or draught, and as the distance of the centre of gravity of the vessel is marked on the model, it can be measured by a rule and the righting lever consequently is known.

For any other angle it is only necessary to repeat the operation, and by adding or subtracting water from the tank the righting lever can be got for any displacement.

A check test can also quickly be made ; for, if the weights be replaced which were taken out of the scale pan in order to balance the upward pressure of the water displaced, and weights are put instead into the other scale pan, then the weights which have to be put in will be exactly half that which was required to be taken out.

This enables any mistake that may have been made to be apparent at a glance and also increases the accuracy of the results as if the values of the righting levers, as given by the two operations, varies slightly, a mean can be got by adding the two together and dividing by two.

This mean has in the experiments been very close to the values obtained by calculation.

In order to find the displacement of the vessel corresponding to each pound of water displaced by the model.

(1.) Mark the deep water line upon the model WL, see Fig. 5 and turn it to the upright position.

(2.) When the tank is empty, find the weight necessary to balance the model when the frame is working upon No 1 steel bar and also when working upon No. 2 steel bar.

(3.) Keep the frame horizontal and fill the tank with water until the model is immersed up to W L.

(4.) Find the weight which has to be taken off in order to keep the machine balanced when working upon No. 1 steel bar and also when working upon No. 2 steel bar.

The sum of the two weights taken off will equal the pounds of water displaced by the model.

(5.) From the displacement sheet get the displacement of the vessel at the load draught.

(6.) Divide this displacement by the weight of water displaced by the model and the result will be the number of tons corresponding to each pound of water displaced by the model.

This can also be found by calculation as the scale of the model is known. Suppose model is made, so far as the sections are concerned, to a scale of $\frac{1}{4}$ in.=1 foot or $\frac{1}{48}$ and that the intervals are taken 24

feet apart and the sections are made of $\frac{3}{4}$ in. boards, then the scale for length$=\frac{3}{4}$ in.$=24$ feet$=\frac{1}{32}\frac{1}{4}$; then pounds of water displaced by model corresponding to one ton of displacement of vessel equal

$$\frac{2240}{48 \times 48 \times 384}$$

It is best, however, to arrive at this from the model itself, by finding with the machine the pounds of water displaced at the load draught, as it takes into account any error due to the making of the model.

DISCUSSION.

Mr. W. Denny: My Lord, it gives me very much pleasure, indeed, to see this further most ingenious development by Mr. Heck of his stability balance. My firm had the honour of helping Mr. Heck in the development of this instrument, and when we saw the balance as he completed it with us, we thought it was not likely to be soon beaten. It has been reserved to Mr. Heck to beat his own best accomplishment. It is perhaps a pity in an instrument of such ingenuity as this that all trace of the great labour of arriving at its perfection is wiped out, so that the history of its mechanical development and of the scientific thought required in it becomes lost to us. I suppose such sacrifice is necessary in scientific progress, to prevent our minds becoming burdened with the methods of the past, and the experiments which precede perfection. When we made the balance at our works, my desire was to have a very large one, so that the measurements could be made upon an open scale. But Mr. Heck then pointed out that the larger the balance the less convenient it would be. You will observe that he has since steadily worked towards reduction and not increase of size, until he even goes the length of suggesting a balance which could be adapted for cardboard sections. The saving in time which has been accomplished by this new apparatus is, I think, very remarkable. Mr. Heck has told us that for cutting his model, and doing the work necessary for obtaining the cross curves of stability, he only requires altogether nine hours. Not more than four years ago the completion of a full set of cross curves for one vessel would have been at least a fortnight's work of a thoroughly skilled man. There is here, then, an encouragement to every shipbuilder in the country to investigate the problem of stability. With this apparatus before us we may congratulate ourselves without in any way reflecting upon our French neighbours, that while they in stability investigations have pursued most beautiful theoretical lines of enquiry, we here have, bit by bit, but very rapidly, pushed the stability question into the front rank of practical possibility. For doing this Mr. Heck deserves a great deal of the credit, as he principally has brought the mechanical investigation of stability to the point that it has attained. I hope the work he has done will not merely remain a permanent record to himself, but will be an example to all the younger members of this Institution, and an encouragement to them to attempt, even in the most difficult questions, a simplification of the difficulties, hoping for the same success which has followed the labours, investigations and experiments of Mr. Heck.

Mr. B. Martell: My Lord, I think the members of this Institution will feel that it is a source of

congratulation to us as a body that a member of this Institution has brought this model to such perfection as he has, and that he would not have undertaken it, and pursued his studies as he has done in perfecting it, had he not had such an opportunity as this Institution affords in bringing it before the public. We have reason, as an Institution, I say, to congratulate ourselves that we have the means, in the case of ingenious and studious persons like Mr. Heck, of enabling them to put before the public the result of such an interesting investigation as this. It is very gratifying indeed to find that Mr. Heck has devoted his time to the carrying on of his investigations and perfecting his model, because there can be no doubt that it will result in very great benefit indeed to those interested in naval architecture and shipping generally. I saw a short time ago an outline of a bill about to be brought forward in Parliament where it was proposed that the captains of ships should pass a surgical examination, that they should be required to walk the hospitals or something of that sort, to render themselves competent to take command of a ship. Now, I think myself, that if some member were to bring forward a bill making it imperative that captains of first-class ships should have one of these models on board, and should learn how to apply it, that would result in far greater benefit. I believe that the intelligence now of a large number of the captains of our ships would be able to apply this, and that it would be highly interesting to them to do it, and would result in very great good in enabling them to know the safety of their vessels as regards their stability under certain conditions of loading. Now, I would go further than Mr. Denny, in suggesting that every shipbuilder should have a machine of this kind in his drawing office; and I hope soon to see the time when every intelligent captain on board his ship will have such a machine, and know how to apply it. I think we are very much indebted to Mr. Heck for the great intelligence and perseverance which he has displayed in perfecting his stability model in the manner in which he has done, and thus given us such a simple, practical and ready means of ascertaining correctly the centre of gravity of a vessel without laborious calculations.

Mr. L. Benjamin: My Lord, I have a few observations which, perhaps, I may be permitted to make with regard to this Paper. I fully agree with Mr. Denny's and Mr. Martell's remarks as to the value of this method, but still I should like to point out one direction in which I believe an improvement could be made. The sectional model, as employed in the instrument, has some disadvantages, and if, by some means or other, the model could be made so as to offer a continuous surface, it would certainly do away with a great source of error. This error consists in the following:—The sectional models are representative of the application of the trapezoidal rule to the results of the calculations of the sections, and, although the latter may be correct in themselves, their summing up in longitudinal direction by means of these or Simpson's Rules will result in a considerable error. Assuming the calculations of the sections to be absolutely correct, as they will be in Mr. Heck's method, if the sections are correct, then this longitudinal error, so far as the displacement is concerned, will amount in many cases to as much as one and a-half or two per cent., as I have found by many calculations which I have made for the purpose, and will result to much more in moment calculations. The same error, of course, is committed when these or Simpson's Rules are applied to the longitudinal summing up of the results of the calculation of the sections when these are made by means of the integrator, and if the latter results agree with those of Mr. Heck's method, it does not follow that this error is

not common to both, as, indeed, it will be. It can only be avoided in the calculation by plotting longitudinal curves from the results of the sectional calculations, and treating the areas of these by the planimeter, and I think it would not be difficult for Mr. Heck to alter his sectional model into a continuous one without giving up the length of the same, as employed at present. By these means the error I speak of would be done away with, and by that the method would be improved. Besides that, I should like to say a few words as to a remark Mr. Martell made just now, proposing to have such a balance as this on board of ships, so that the captain might experiment with it. I do not believe that would lead to anything very valuable, because the captain has to work with the ship as loaded. The difficulty which he has, gentlemen, is to find the centre of gravity of the ship, not the centre of buoyancy, or metacentre; but Mr. Heck's apparatus gives you only data in connection with the centre of buoyancy, or the metacentre. Two years ago, when I read my Paper on stability before this Institution, I brought forward a balance, which I described at greater length shortly afterwards before the Royal United Service Institution, and which would help the captain or shipowner to determine the centre of gravity of his vessel under the conditions of any given load, after the stability curves which are due to the vessel's form are determined; but I had to drop the practical use of it because I found that captains, as a rule, would not be able to use it. The calculation, for the curve of stability, should remain in the province of the drawing office alone, and the captain should get nothing more of it than the results. I even am inclined to think that he should not get the stability curve itself, but simply the results in a tabulated and easily comprehensible form, such as Mr. Denny mentioned here in the discussion of Professor Elgar's Paper two years ago; informing him of what his vessel can do under certain circumstances.

Mr. Schönheyder: I should just like, my Lord, to point out what appears to me to be a small defect in that model, but perhaps Mr. Heck can explain it away. I do not quite understand why those knife edges should be so much out of level. It is a well known thing that the pin edges of the best balances should be in the same plane, or nearly so, in order to get accurate results. In order to get true weighing, you must have the model perfectly level. A slight variation of the level will make your results inaccurate. There is another suggestion I would make. It seems to me a laborious method to have to lift up the whole of that frame and turn it end for end. It would seem to be an advantage to have the carrying blocks over the form moveable, so as to shift them alternately under either one or the other set of knife edges, instead of having to shift the whole frame with the model attached.

Mr. J. H. Heck: My Lord, in reply to the suggestions of the last gentleman, no doubt if all the knife edge bars were in one plane, the sensibility of the balance would be increased and render possible the determination of very small differences of weight. Originally the machine was so arranged; it had, however, to be altered so as to increase the stability of the balance, and the vertical position or relation of the knife edge bars in this machine is what experience has proved necessary. I agree, however, that turning the balance end to end, while simple, is perhaps not the neatest or easiest way of doing what is required. The idea of working this method out only suggested itself some two months ago, and I have, therefore, not been able to put it before this meeting in so

complete a manner as could otherwise have been done, while at the same time ease and speed were to a certain extent sacrificed in order to secure simplicity. Members, however, can easily make a machine for themselves at the cost of a few pounds, and I have no doubt that if this is done the arrangement will be improved, and that whenever shipbuilders generally come to construct stability curves this method will be largely employed. Mr. Benjamin made some observations on the remarks of Mr. Martell, and as my opinion coincides with that of Mr. Martell, perhaps it would not be amiss if I just direct a little attention to that. One benefit which this mechanical method has conferred, and which has been pointed out to me by many persons, and I may mention one, viz., Professor Elgar, is the ease with which the principles by which stability is governed can be made clear to those who know very little of the subject, and I therefore firmly believe that the models and apparatus would be useful for giving seamen an intelligible idea of the fundamental basis on which a vessel's stability depends. Quite recently, at a meeting of the North East Coast Institute of Engineers and Shipbuilders, I pointed out that sailors find out their position at sea by certain rules which, while they know how to apply them, not one captain in a hundred can prove, so in a like manner it is not at all necessary that mariners should grasp all the scientific aspects of this question of stability in order to enable them to profit by the knowledge which an elementary acquaintance of the principles of stability would certainly give them as to how a vessel's safety is affected by the stowage of cargo. Bad stowage, or stowage effected by men who are ignorant of the elements of stability, is the cause why many vessels have to be posted as "Missing." In reference to the statement that increased accuracy would be got by a continuous, in place of a contracted model such as is here, I may say that two and a-half years ago, when at Mr. Denny's establishment, a member of that gentleman's scientific staff, Dr. Amsler, made some investigations in order to see what effect the employment of the trapezoidal rule in the longitudinal direction had upon the results, his calculation practically confirmed mine that the difference is but trifling, and experiments clearly indicate that a continuous model does not give an increase of accuracy over a contracted one. Again, if reference is made to the experiments appended in last year's Paper, and also in this one, it will be seen that the difference between the calculated results and those obtained by the model are of no practical magnitude, and, in fact, no two individuals calculating curves of stability for the same vessel will get results within the two per cent. mentioned by Mr. Benjamin. One more point in my Paper—it is stated that six hours is required to make a model and nine hours to find the weights from which cross curves can be made, after two or three experiments with the balance one gets more expert, and I therefore think fifteen hours a fair average time within which to make a model and to get out complete cross curves of stability. I have only, in conclusion, to thank you for your kind reception of my Paper.

The PRESIDENT: I am quite sure, by the sound which has just reached me, that you will allow me to convey your united thanks to Mr. Heck for the labour and research which he must have brought to bear in perfecting this very valuable machine. I am extremely glad also to add this, because I can speak from my own personal knowledge that the jury at the South Kensington Exhibition of Inventions, with which I had the honour of being associated last year, conferred on Mr. Heck the highest possible mark of their approval in the shape of a gold medal for this extremely valuable model.

A STRAIN INDICATOR FOR USE AT SEA.

By C. E. Stromeyer, Esq., Member.

[Read at the Twenty-Seventh Session of the Institution of Naval Architects, April 14th, 1886; the Right Hon. the Earl of Ravensworth, President in the Chair.]

Ever since the introduction of iron in the place of wood for ships and stone for bridges, the study of the strains and stresses set up in these structures has engaged the thoughts of nearly all the great mathematicians, and has led them to draw conclusions, which in most cases are fairly correct, yet, even those which rest apparently on the soundest foundations are only quite correct for conditions which rarely, if ever, occur in practice. If it were otherwise there would be no necessity for the use of a factor of safety, which, where life and property are concerned, is never less than four and sometimes very much more than ten.

It will be remembered that the first determinations of stresses in a bridge were of a practical nature, the various tie-bars and struts of a model bridge were successively replaced by a spring balance, carefully adjusted for each load, so that its length was then equal to that of the absent bar. These experiments very soon led to mathematical investigations, which are still employed, and which follow the same idea, viz.:—the various bars are consecutively supposed to be severed, and instead of using a spring balance, mathematical equations are employed to determine the forces which must act in these bars to keep them in equilibrium.

Another method for obtaining the same information, but one which is more difficult of application, though more fruitful in results, is to consider the deformations which take place when a structure is loaded, and from them deduce the stresses which cause them. This would at first sight appear to be an impracticable and roundabout way, were it not that several problems, for instance those connected with the bending of beams, and straining of thick cylindrical shells, could not have been solved in any other way. There are yet many questions waiting to be dealt with, in fact the determination of stresses in flat plates has hardly been attempted, while as regards the determination

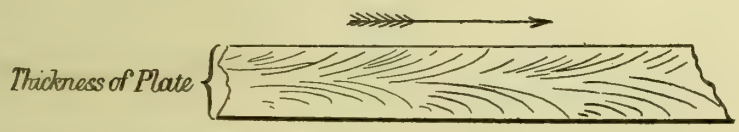
of stresses in solids, nothing has been done except the construction of half a dozen fundamental equations by M. Lamé, which although not incorrect, are not comprehensive enough to lead to results; otherwise, as far as I know, nothing has been attempted in this line. Some of the difficulties which surround this subject will, I hope, vanish as soon as it is possible to treat this subject experimentally, this is one of the reasons which prompted me to devise an instrument with which to obtain reliable measurements of strains, and I hope, as it has shown itself eminently suited to determine the strains in ships, that a short description will be of interest.

First, however, it will be necessary to make a few remarks on another instrument, intended for the same purpose, but impracticable on account of its extreme sensitiveness and the difficulty experienced in working it out of doors, because it led me to a few discoveries which deserve being mentioned. The instrument, now about five years old, is based on a phenomena called the interference of light. A similar method had been adopted by M. Fizean to determine the co-efficients of expansion of solids; so sensitive was it found to be, that he could easily measure the different rates of expansion of the various axes of crystals with an accuracy, which reduced probable errors to 1 per cent. This extreme sensitiveness compelled me to use only very short spanned instruments ($\frac{1}{2}$ in. to 3 ins.), and they could therefore not be used to ascertain the average strains over long spans, but only to investigate local changes of strains: but for this purpose no other method could give more accurate results.

The discoveries I referred to are the following:— The circumferential strains are not uniformly distributed over the lengths of boiler shells, they are less near circumferential seams than in the centres of plates, and are considerably less near the ends where the shell is riveted to the flat end plates. A little reflection will show that this is only natural, and, I think, that some engineers, who, independent of my experiments, have arrived at the same conclusions, argue that the circumferential seams should be looked upon as strengthening rings, and that therefore a shell is really stronger than the ordinary calculations would show it to be. Now, although I grant the correctness of these views, further experiments have shown me that these circumferential seams are a source of weakness, for I have found that the local strains in the solid plates on either side of a lap joint are four to five times greater than the ordinary calculations suppose them to be, or about eight times greater than in the other parts of the solid plates, so that one could almost expect these points (adjoining the lap joints) to tear long before the joints have been

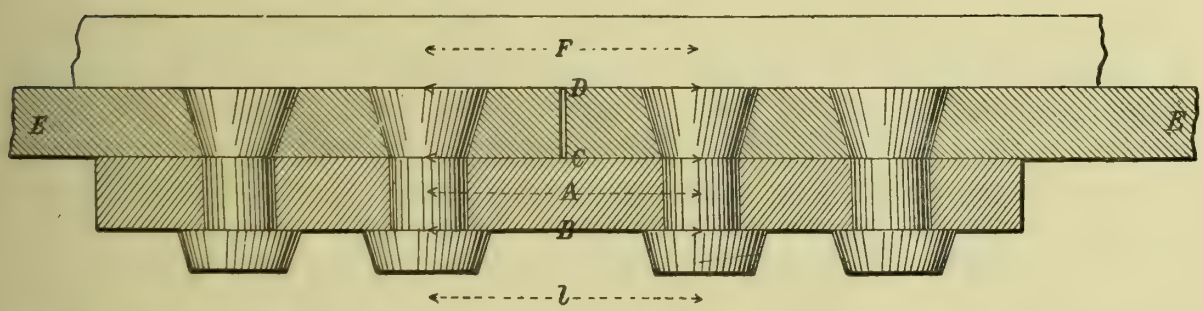
strained to their utmost. This probably accounts for the tearing of solid plates when boilers are burst experimentally. It usually happens that both the lap joint and the solid plate tear instead of only the former. In future cases it would be interesting to know whether the fracture commenced in the centre of the seam, where it is unsupported by the adjoining strakes, or whether it started in one of the adjoining solid plates. An examination of the fracture will show in which direction it travelled. Feathery groupings of the crystals, as per sketch, will indicate that it travelled in the direction of the dart.

FIG. 1.



The same phenomena of severe local strains was observed on a ship's side; it was found that they were about twelve times greater just above or below the butt straps than anywhere else. The reason is not far to seek. The seams, especially single butt strap joints, are far more elastic than the adjoining solid plates, and the greater part of the strains naturally get thrown on the latter. The following sketch will make that clear :—

FIG. 2.



The amount of stretch in a length (l) at the various points is as follows; E, the stretch in the solid plate, being taken as the unit of comparison :—

A = + E	C = + 7E
B = −5E	D = + 19E

It is therefore quite natural that the adjoining plate should be strained most severely at F.

Reverting to the case of the boiler, it has often occurred to me whether the shells would not be stronger if the longitudinal seams were carried from end to end, as is sometimes done in donkey boilers.

These and similar questions are, of course, of the greatest importance, but most of the rules of construction are based on other practical considerations, and take no further notice of these unknown quantities than to include them in the factor of safety, and I have only drawn attention to them in order to point out how much yet remains to be learnt.

The instrument to which I wish more particularly to draw your attention is not nearly so sensitive, but it is capable of giving accurate results, and, if necessary, in the form of diagrams, and, as there are very few adjustments to make, these cannot well get out of order. One of the various uses to which the instrument can be put is to determine the modulus of elasticity of test pieces; the principle of all the other adaptations is the same as in this case, and a detailed description will make it clear.

Two flat plates, T, B, Fig. 3 (Plate IV.), about $1\frac{1}{2}$ in. wide and of any length, are pressed together by means of two springs, S, S, in such a manner that one plate projects at one end and the other at the other end. Fixed centrepoints, C, C, are screwed into these ends, and a graduated dial, D, is attached to the upper one of the two plates T. Two of these instruments are held together by a pair of clamps, K, K, fixed just over the centre-points, which, when screwed tight, press the centres against both sides of the test pieces; for safety against slipping a few taps of a hammer embed them more firmly. Then, when everything is ready, a pair of very fine hardened steel wire rolling pins, P, P, to which light pointers have been attached, are inserted between the plates. These rolling pins, when in position, should be in the centres of the dials. On applying the load to the test piece elongation takes place, the centrepoints move slightly away from each other, carrying the plates with them, which, as they move in opposite directions, and as they are held apart only by the interposed rolling pin, cause it to revolve, and the angle through which it has moved can then be read off with the help of the pointer and dial. I have obtained the best results with a wire which was drawn for me, and which measures exactly ·015 inch in circumference. The dial is divided into fifteen equal parts, and their decimals, so that one division represents one thousandth of an inch, and variations as small as twenty thousandths of an inch can be detected.

The following table contains the readings recently taken with these instruments and a few calculations based thereon.

TABLE I.

STEEL TEST PIECE, ¾ INCH DIAMETER. DISTANCE OF CENTRES 8·05 INS.

Readings.	I.					II.					III.				
Instruments.	1		2			1.		2.			1.		2.		
Stresses.	Readings.	Differences.	Readings.	Differences.	Sum of Differences.	Readings.	Differences.	Readings.	Differences.	Sum of Differences.	Readings.	Differences.	Readings.	Differences.	Sum of Differences.
lbs. 0	17·07	...	16·85	..	...	16·98	...	15·89	.	...	17·41	...	16·20	...	...
2,000	17·30	—·23	15·34	+1·51	1·28	16·87	+·11	14·97	+·92	1·03	17·29	+·12	15·27	+·93	1·05
4,000	16·77	+·53	14·88	·46	·99	16·26	·61	14·58	·39	1·00	16·73	·56	14·82	·45	1·01
6,000	16·12	·65	14·50	·38	1·03	15·63	·63	14·18	·40	1·03	16·09	·64	14·42	·40	1·04
8,000	15·43	·69	14·13	·37	1·06	15·00	·63	13·79	·39	1·02	15·41	·68	14·01	·41	1·09
10,000	14·80	·63	13·75	·38	1·01	14·39	·61	13·35	·44	1·05	14·85	·56	13·62	·39	·95
12,000	14·19	·61	13·30	·45	1·06	13·82	·57	12·93	·42	1·01	16·23	·62	13·14	·48	1·10
14,000	13·61	·58	12·88	·42	1·00	13·24	·58	12·50	·43	1·01	13·61	·62	12·70	·44	1·06
16,000	13·05	·56	12·43	·45	1·01	12·79	·45	12·20	·30	·75	13·03	·58	12·32	·38	·96
18,000	12·65	·40	11·99	·44	·84	12·39	·40	11·71	·49	·89	12·61	·42	11·89	·43	·85
20,000	12·31	·34	11·52	·47	·81	11·89	·50	11·25	·46	·96	12·00	·61	11·45	·44	1·05

Average difference of readings per 2000 lbs., I. . . . . 1·009
Stress per 16·1 inch between centres . . II. . . . . ·973
III. . . . . 1·018

Total . . . 3·000 mean 1·000

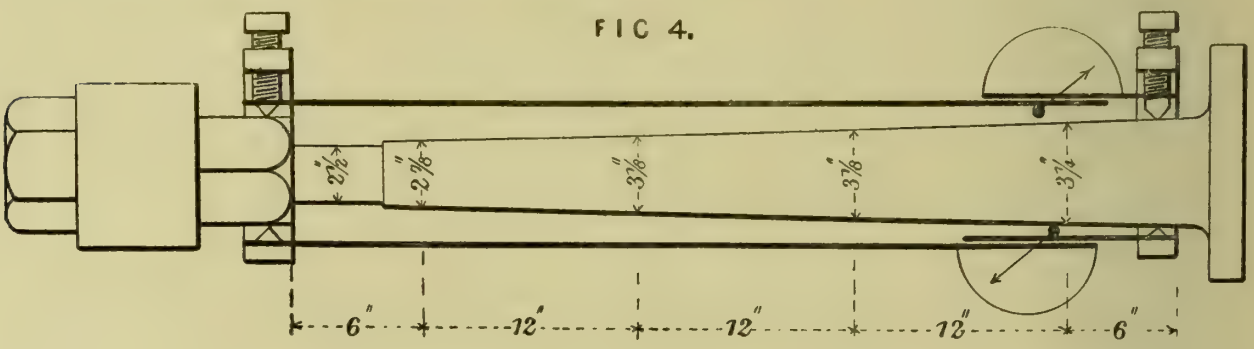
$$\text{Modulus of elasticity} = \frac{\text{Stress}}{\text{Strain}} = \frac{2000}{1{\cdot}000} \times 16{\cdot}1 \times 1000 = 32{,}200{,}000$$

This experiment shows the reliance which can be placed on the accuracy of the instrument. It will be noted that the readings of the two instruments are not equal, and even irregular though the sum of differences is practically constant. This is due to the difficulty of getting the centre of pull of a testing machine to pass exactly through the centre of the test piece. If these do not coincide, the one reading must necessarily be larger than the other. If in a square bar the pull is only one-sixth of the diameter nearer to one instrument than the other, the one indicates double the mean strain, and the other one nothing at all.

I was forcibly reminded of this on one occasion while measuring the strains on an arched wrought iron rib of a bridge, while it was being tested with proof loads. Nine instruments in all, viz. : three at three different cross-sections were attached to the top, bottom, and centre of the web (the section was I), but all on one side, so as to guard against interference from the wind which was blowing on the other. The span of each instrument was 10 ft., and as vertical angle irons were fitted to the web the instruments had to be fixed 3 ins. away from it. Consequently any slight tendency to buckle, due to the centre of web and effort not being identical would be greatly exaggerated. This would chiefly affect the mean stresses. This had not occurred to me while making the experiments, and I was therefore greatly surprised to find that although the readings of the three instruments at each section agreed amongst themselves, and although the centres of efforts at these three sections agreed absolutely amongst each other, the total thrust was decidedly different at the three sections, and differed seriously from the calculated one.

A problem nearly related to the determination of the elasticity of a test piece is the measurement of the propeller thrust in one of Mr. Gourlay's engines. In these engines the thrust block is always secured to the bed plate by two struts. To one of these a pair of instruments four feet long were clamped as shown in Fig. 4.

FIG 4.



Readings were then taken while the engines were going at various speeds and when they had stopped. The results are contained in Table II.

TABLE II.

Propeller Thrust.

Vessel's Speed. Knots.	Revolutions of Engine.	Reading of Instruments in thousandths of an inch.		Sum of Differences. Thousandths.	Calculated.	
		No. 1.	No. 2.		Thrust tons.	Effective Horse power.
0	0	12·15	10·20	...	...	...
12	72	13·8	11·00	2·55	4·14	336
0	0	8·85	12·35	...	...	...
?	67	9·20	13·20	1·20	1·95	?
0	0	5·50	9·10	...	...	...
?	65	6·60	9·60	1·60	2·60	?
0	0	5·60	9·30	...	...	...
?	56	6·80	9·50	1·40	2·27	?

The sum of the mean sectional areas of the two struts is 14·56 square inches. Length between centres 2 × 4 ft. = 96 inches. Modulus of elasticity assumed to be 24,000,000. The indicated horse-power was said to be about 720, so that the efficiency would be under 50%.

In the following experiments on strains in ships and bridges the spans of the instruments were from 10 to 20 feet long. This necessitated the use of a wire stretched tight by a spring instead of a lengthening bar. As the spring could be made to pull downwards, there was no use for the vertical springs, S, over the top plate T. The bottom plate B was replaced by a short piece of ⊏ iron of which one flange was bolted to the structure and the dial to the other one, while the web formed the under surface for the pin to roll on. With this arrangement all readings must be corrected on account of the elasticity of the wire, for the entire stretch of the span is not taken up entirely by the spring but partly by the wire. However, by using strong wire and long springs this error can be reduced to less than one per cent. The friction of the rolling pin is so small that it can safely be neglected.

The following are the results of a few observations while launching vessels. In the case of the S.S. "Idas" and S.S. "Albatross" an observer was stationed at each instrument while no observations were taken on H.M.S. "Benbow" during the launch, and

the readings of instruments 1 and 9 was considerably deferred on account of the bunker doors having been closed for two hours.

TABLE III.

Longitudinal Launching Strains S.S. "Idas" (Iron). *See* Figs. 17 (Plate V.) and 19*a* (Plate IV).

Number and Position of Instrument.	1 Main Deck.		1 'Tween Deck.		3 Bilge Knee.	
	Strain. $\frac{1}{10000}$.	Stress. lbs. per sq. in.	Strain. $\frac{1}{10000}$.	Stress. lbs. per sq. in.	Strain. $\frac{1}{10000}$.	Stress. lbs. per sq. in.
Assumed initial strains .	0	0	0	0	0	0
Launching strains .	+ ·127	+ 250	0	0	+ ·038	0
,, ,, .	− ·127	− 250	0	0	+ ·038	+ 75
,, ,, .	+ 1·900	+ 3750	− ·019	− 37	+ ·051	0
,, ,, .	+ ·640	+ 1250	+ ·140	+ 275	+ ·051	+ 100
,, ,, .	+ 1·900	+ 3750	− ·242	− 470	− ·420	− 825
Floating strains . .	+ ·127	+ 250	+ ·013	+ 25	− ·102	− 200

In calculating the stresses a modulus of elasticity equal to 24,000,000 has been assumed. The sign + signifies elongation or tension, — signifies contraction or compression.

TABLE IV.

Longitudinal Launching Strains. S.S. "Albatross," (Steel) Units 1 per 10,000. *See* Figs. 18, 18*a*.

Number and Position of Instrument.	Upper Deck.				Main Deck.	'Tween Deck.		Bilges.
	I.	V.	VI.	Port. VII.	II.	VII.	VIII.	IV.
Assumed initial strains .	0	0	0	0	0	0	0	0
Launching strains . .	+ ·29	+ ·88	+ ·27	+ ·77	+ ·10	0	+ ·008	− ·10
,, ,, . .	− ·02	− ·30	− ·02	− ·60	0	0	− ·006	0
,, ,, . .	+ ·05	− ·30	+ ·10	+ ·53	0	0	+ ·008	− ·10
,, ,, . .	0	− ·30	+ ·10	+ ·53	0	0	− ·007	+ ·06
Floating strains . . .	0	− ·30	+ ·10	+ ·53	0	0	+ ·008	− ·08

Calculated Stresses in Pounds per sq. inch.

Assumed initial stresses .	0	0	0	0	0	0	0	0
Launching stresses . .	+ 580	+ 1760	+ 540	+ 1540	+ 200	0	+ 16	− 200
,, ,, . .	− 40	− 600	− 40	− 1200	0	0	− 12	0
,, ,, . .	+ 100	− 600	+ 200	+ 1060	0	0	+ 16	− 200
,, ,, . .	0	− 600	+ 200	+ 1060	0	0	− 14	+ 120
Floating stresses . . .	0	− 600	+ 200	+ 1060	0	0	+ 16	− 160

In calculating the stresses a modulus of elasticity equal to 32,000,000 has been assumed. The instrument VI. was placed above an opening in the ship's side.

TABLE V.

Transverse Launching strains, H.M.S. "Benbow" (Steel). *See* Fig. 19. Plate V.

Units of strains: 1 per 1,000,000.

Number of Instrument.	1. Starboard Bunker.		2. Starboardstokehold.		3. Starboardstokehold.		7. Port stokehold.		8. Port stokehold.		9. Port Bunker.	
Time of Experiment.	Strain.	Stress. lbs,	Strain.	Stress. lbs.	Strain.	Stress. lbs.	Strain.	Stress. lbs.	Strain.	Stress. lbs.	Strain.	Stress. lbs.
8 a.m.—9 a.m.			+6·4	+194	−3·7	−110			−35·2 ?	-1056 ?		
9 a.m.—2 p.m.	+1·3	+39			0	0	−22·5	−670			+ 4.5	+135
1 p.m.—2 p.m.	−1.5	−45	−21·3	−640	+6·1	+173					−35·7	−1070
2 p.m.—3 p.m.	−2·1	−63	−3·0	−90	+4·8	+144	−1·14	−34				
3 p.m.—3.15 p.m.			+7·0	+210	−2·5	−75	0	0				
2—4 p.m.	+120	+3600	+4.0	+120	+2·3	+70	−1·14	−34			+45·5	+1360

TABLE VI.

Calculated Launching stresses in Engine-room Bulkhead, H.M.S. "Benbow."

Instruments 4, 5, 6.

Time of experiment.	Vertical stress. lbs.	Horizontal stress. lbs.	Horizontal steering stress. lbs.
11 a.m.—1 p.m.	−1,330	−465	−730
1 p.m.—2 p.m,	−115	−59	+99
2 p.m.—3 p.m.	+141	+68	+520
3 p.m.—3.15 p.m.	+660	−294	+151
2 p.m.—4 p.m.	+780	−223	+880

In the two last tables the strains are given in millionths. The stresses are calculated on the assumption that the modulus of elasticity is 32,000,000. The readings of the instruments on the engine room bulkhead have not been given, as it would require too long a mathematical explanation to show the relation which exists between them and the calculated stresses. In the present case this would be particularly useless, as on account of the thinness of the bulkhead plate the readings do not represent the actual strains, and the above Table has only been added to show that, with the help of three determinations of strains, all the stresses which cause them can be calculated.

An occasion presented itself for measuring the docking strains in the S.S. "Hooper" (now "Silvertown"). Four instruments had been fitted on board the evening before

undocking, one between each deck, and as close along the shell plating as possible. The position of the instruments will be seen on reference to Fig. 8, Plate V.

Early next morning, while the water was being let into the dock, the vessel's draft and the ever-varying positions of the five pointers was noted. These readings have been plotted down in two diagrams, Figs. 9 and 10, Plate VI. The abscissa of the first one gives the mean draft, while that of the second one gives the difference of draft; the time is also noted. With the help of diagram Fig. 10, the actual bending moment of the ship could of course be calculated, but as I was unable to obtain the water-lines, and could consequently not calculate the pressure with which the keel rested on the block; no comparison could be made, and the calculation has therefore not been carried out. Possibly the result would have been incorrect, because the sun made its appearance at about 8.30 a.m. and had a visible effect on the motion of the pointers. The left part of diagram Fig. 9, containing the strains from the first inlet of water were used for the purpose of finding the actual neutral axis of the midship section. It was found to be 173″ above the tank top—the probable error not exceeding 10″. A calculation of the midship section (no deduction for rivets) placed it at 168″ above the tank top. This does not include the light superstructure, which would probably raise it a few inches higher. These results show that observations of this sort can be made with tolerable accuracy. Change of temperature, especially if the wires, as in this case, cannot adapt themselves to it, will always disturb the readings. In the present case a difference of only 1° Fahrenheit between the ship's plate and the wire would cause the pointer to move through one division. It must also be remembered that the gradual heating of the upper part of the vessel not shielded by the sides of the dry dock would slightly elongate the ship and cause all the pointers to rise, instead of the upper ones rising and the lower ones falling. Every unit in the diagrams, Figs. 9 and 10 is equal to a stress of about 310 lbs. per square inch.

Another subject to which the instruments can be applied with great advantage is the measurement of strains in ships in a seaway, but, to be fruitful of results, means should be provided for comparing them with the waves which produce them. I therefore beg to submit the following proposal, which, as far as I have been able to test it, is not attended with any difficulties of observation and requires only a stop watch for taking the time.

Mathematical considerations show that the sine of the angle of the maximum wave slope of a trochoidal wave is equal to the ratio of the two radii which produce it. It is, therefore, only necessary to measure the wave slope and the length of the wave, when the height can be

calculated. The length, 2L, is easily measured. Note the time which elapses between the passage of two wave crests; call it t_1. Note the time which elapses between the instant that the back slope of an advancing wave can just be seen and the time when its crest passes under the observer. Call this time t_2: then if the height of the observer above the water level is H, the tangent of the wave slope will be approximately equal to the following expression, $\frac{H}{L}\frac{t_1}{t_2}$. For angles above 15°, which correspond with short steep waves, this value is very nearly equal to the tangent of the angle of the wave slope, but for smaller values the following table has been compiled :—

TABLE VII.

Table for Calculating Wave Heights.

$\frac{H}{L}\cdot\frac{t_1}{t_2}$	$\frac{\text{Height of Wave.}}{\text{Half length of Wave.}}$
·0206	·0056
·0405	·0111
·0590	·0167
·0795	·0222
·0990	·0278
·118	·0332
·136	·0388
·153	·0443
·172	·0498
·189	·0553
·207	·0606
·225	·0661
·241	·0716
·258	·0770
·275	·0825

The first experiments to determine the strains in a ship at sea were made on board the S.S. "Compton" (Fig. 7*a*, Plate IV., and Fig. 7, Plate V.), on a voyage from London to Cardiff. The wind calmed down before Dover was passed, and the highest waves that were subsequently met with did not exceed 4 feet. Seven instruments were fixed to the ship's side, as shown in Fig. 7, 7*a*. Readings were always taken simultaneously at two instruments, so as to compare the relative longitudinal strains at the various points. The results are contained in Table VIII. It was noticed distinctly that the maximum strain was indicated when a wave passed under the instrument.

Table VIII.

Difference of highest and lowest Position of Pointers of two Instruments read simultaneously.

Number and Position of Instruments.		A. Top Fore Hold.	E. 'Tween Deck.	A. Top Fore Hold.	F. Bottom Fore Hold.	A. Top Fore Hold.	C. Bunker.	A. Top Fore Hold.	G Engine Bilges.
No. of Readings	1	+ ·60	+ ·04	+ ·50	− ·15	+ ·25	+ ·40	+ 1·80	− ·30
,,	2	+ ·65	+ ·06	+ ·80	− ·10	+ ·25	+ ·40	+ 1·40	− ·10
,,	3	+ ·70	+ ·06	+ ·80	− ·10	+ ·25	+ ·40	+ 1·10	− ·15
,,	4	+ ·10	+ ·06	+ ·50	− ·10	Mean ratio		+ 2·70	− ·40
,,	5	+ 1·00	+ ·04	+ 1·00	− ·10	·625		+ ·30	0
,,	6	+ ·90	+ ·07	+ ·70	− ·10	+ 1·10	+ ·60	+ ·80	− ·20
,,	7	+ 1·40	+ ·06	+ ·40	− ·10	+ ·70	+ ·60	Mean ratio	− 7·04
,,	8	+ 1·20	+ ·06	+ ·60	− ·15			A	D
,,	9			+ ·30	− ·10			+ ·20	+ ·10
,,	10			+ ·50	− ·15			+ ·10	+ ·15
Mean ratios.		+ 14·6		− 5·3		+ 1·5		+ 1·2	

The first three readings of instruments A. and C. were read together while passing through a head sea. The two last were read in a beam sea.

The dials were divided circumferentially into inches and decimals. Every inch in the above table, which gives the actual readings, is equal to a stress of about 250 lbs. per square inch. A. E. and A. F. were read when the highest waves appeared to be about 4 feet in a head sea.

The strains of ships in a beam sea, provided she does not roll too much, are of special interest, as they give a fair indication of the amount of initial straining of the ship. This will be better understood by considering a ship in the trough of a very long swell. Suppose a strong spring balance with 100 lbs. attached to it, be hung up as near as possible to the centre of gravity; then, with the up and-down motion of the ship, this weight will sometimes appear to weigh, say 110 lb., and then 90 lb. What is true for this one weight is true for all the weights in the ship; they vary 10 per cent., more or less. The strains which they produce are read off on the instruments. And now suppose, that the sheerstrake

amidships shows 200 lbs. variation of stress when the spring balance shows 20 per cent. variation of weight, then the assumption that the total weight of the ship produces 2,000 lbs. stress in smooth water will not be very incorrect.

It would have been very interesting if these various particulars could have been fixed in diagrams, so that simultaneous variations of all these stresses could be compared. Instruments have been prepared for this purpose, and have been fitted on board, but the sea has on each occasion turned out to be so calm that practically no results have been obtained.

No difficulty was experienced in taking strain diagrams of ships while being launched and of girders of railway bridges. These will be found on referring to Fig. 5, Plate IV., and Figs. 11, 12, 13, 14, Plate VII.

In these cases the same instruments, but without dials, were used ; thicker wires and stronger pointers, with very soft 6B black lead pencils, were inserted between the plates : sheets of metallic paper were passed in front of these pencils on which the diagrams were then recorded.

Both the vessels which were launched and the three trains which passed over the bridge are placed in such a relative position to the strain diagrams which they produced, that if tracings of the bridge or ship and the scale above it are made, and these passed along the base line of the diagrams, the relative position for the various strains will at once be seen.

In conclusion, I beg to point out several uses to which the instrument could be put for furthering the science of Naval Architecture in particular, and the investigation of strains in solids in general.

1. The determination of the relative value of the different parts of vessels for giving longitudinal strength, tranverse strength and strength against raking strains.

2. The influence of the loading and of the shape of a vessel on the distribution of strains.

3. The influence of various systems of waves on the strains in ships.

The most favourable conditions for making such experiments would be found on board of ships carrying pig iron, copper ore, or other very heavy cargoes, as in those cases the sides of the ship are accessible.

DISCUSSION.

Mr. Macfarlane Gray: I cannot say anything about this, except that we have before us to-day, in Mr. Heck's and Mr. Stromeyer's Papers, two very remarkable illustrations of what powerful results can be obtained from very simple means, such as this bit of fine wire which Mr. Stromeyer uses. What he proposes to do with it is very beautiful. The instrument will, doubtless, become more manageable than it is now, and as great improvements will no doubt be yet made in it as Mr. Heck has made in his invention since last year; and probably we shall also get very valuable results from its application.

Mr. J. H. Biles: My Lord and Gentlemen, this Paper is one which, I think, we cannot thank Mr. Stromeyer too much for bringing to the notice of this Institution. The question of the strains of ships, in all conditions, is one which is of the greatest importance to the Naval Architect. We are at present in the habit of designing ships upon the results of experience and the calculations that we are able to make upon theoretical bases, but here we have brought to our notice an instrument which enables us to determine the actual strains which are brought upon ships, and not to rest our calculations and dispositions upon theoretical considerations alone. As this question of the strength of a ship is one which involves the thickness of the different parts, and in consequence involves the weight of the structure, it is one which lies at the root of the whole question of ship designing. Any weight (of course it is unnecessary to say it) that can be saved in the structure of a ship is made available for the purpose for which the ship is constructed—to go at the highest speed, to carry cargo, or whatever purpose may be necessary. Therefore this question of recording the strains brought to bear upon ships is one of the utmost importance to us. The manner in which it has been worked out, the painstaking and careful and accurate way in which it has been done, is one for which we cannot thank Mr. Stromeyer too much, and I think it will be well if all those interested and able to take up this question will do it and follow it up with a view to obtaining as much information on the subject as possible. It is a little difficult at first sight to discuss a paper of this kind, but there is one thing that I should like to ask Mr. Stromeyer, if he would be so good, to add to his Paper, that is, some of the particulars in connection with the launching of the ship, more than have been given now. It must be well known to all who have considered the question of the launching of the ships, that the strains that are brought on them must depend very largely on the amount of buoyancy that is necessary, or is brought to be bear, by the upward force of the water to lift a ship, when she is launching, and also by the weight of the ship. Therefore I think, as we have an accurate record here of the strain brought to bear on the ship, we should also have, if possible, some information to enable us to determine what the actual forces were that were acting upon the ship at the time. If Mr. Stromeyer can add that information to his paper, I think it will enable us to correlate that information to other information which may be existing at the present time. Further than that I have no more to say, except to thank Mr. Stromeyer for the very valuable Paper which he has brought before this Institution.

Mr. R. Ramage: My Lord, I have listened with very great pleasure to the paper read by Mr. Stromeyer, and I have only a word to add. I think the questions contained in this Paper are much more questions for Lloyd's Registry than for shipbuilders, and for this reason. We know that nearly 90 or 95 per cent. of all the ships that are built are built in accordance with Lloyd's Rules. Shipbuilders simply have to follow those rules, in carrying out their scantlings, and the other directions pointed out in the rules. Therefore, it is not a question for the shipbuilder at all, to test the strains on a vessel which is going to be launched. That is a thing which must be ascertained by Lloyd's and made clear to shipbuilders, who are building to Lloyd's Rules. I wish to point out that the questions in this paper are more for the consideration of Lloyd's Registry than for shipbuilders, excepting for vessels not classed.

Mr. John Corry: I am sorry that any member of this Institution should be found to come forward and say that such investigations as these should only be taken up by Lloyd's. The responsibility, no doubt, rests with us (I speak as a member of Lloyd's Committee), to see that proper rules and calculations are made, which will stand the test of practical work. At the same time, I think it equally incumbent on all shipbuilders to do their best to help Lloyds', or any other Committee, in such investigations. I am happy to say that Mr. Stromeyer, as also the gentleman who read the previous paper, are members of our staff, and I think it will be acknowledged that they both do us credit. I think any one who examines the instrument of Mr. Stromeyer must be struck with its extreme simplicity, and with its capability of measuring with great accuracy minute changes in material when tested, equally with those which may occur in large structures, such as ships, bridges, &c. You can test by it—what I believe is one of the most important points, and which I think, perhaps, has not been sufficiently investigated up to this time—the limit of elasticity of all materials. I think the crucial point in the investigation of the strength of all structures is to know the limit of the elasticity of the material used, because, if any structure, especially that of a ship or bridge, is strained beyond that point, its strength as a structure is seriously impaired, so that it is most important that we should be able to ascertain, with the minutest accuracy, what the limit of elasticity is, and which this instrument is admirably adapted to determine. I have nothing special to add except to thank Mr. Stromeyer for the great trouble he has taken in investigating this difficult problem, one which, I believe, by means of such an instrument, will be much more generally understood than it is at present.

Mr. J. Inglis, Junr.: I beg to differ entirely from Mr. Ramage, as I for one am not yet prepared to completely efface myself as a shipbuilder, and put myself under the paternal care of Lloyd's Committee, much as I may respect the gentlemen who compose that body. We are frequently asked to design vessels for special purposes, which the interference of Lloyd's would only spoil and render totally unfit for these purposes; it would in fact be impossible to construct them at all under Lloyd's Rules. Vessels of light draught for rivers and other localities where the depth of water is limited, Lloyd's do not pretend to legislate for. But these vessels are sometimes in positions where they are severely strained, although they are not sea-going; these strains have to be provided for, and I would welcome any instrument or appliance which might help us in checking the results of our calculations.

Mr. W. Denny : I think the point Mr. Inglis has raised is one of the very highest importance. It would be deplorable if the shipbuilders of this country allowed their intelligence and enterprise to be paralyzed by the supremacy of Lloyd's. Mr. Ramage's remarks indicate a tendency in this direction, a tendency which I hope, from one quarter or another, will sooner or later receive a check. There is no doubt construction is falling more and more absolutely into the hands of Lloyd's, and that there is a very serious danger to progress in construction, in this very large department of naval architecture, falling so absolutely into the hands of one body. These remarks have no reference to Mr. Stromeyer's paper, but only to the point raised by Mr. Ramage. We, in this Institution, have the spirit of our profession sufficiently at heart to recognize a good gift, from whatever source it comes, and we shall all agree that although Lloyd's may have and must have a dangerous tendency from their very organization to become dogmatic and absolute, the danger is somewhat lessened by the presence on their staff of several young and enterprising naval architects. I have listened to Mr. Stromeyer's paper with very great interest. The work he is doing is still in process, but the wonderfully simple principle on which he proceeds, that of greatly exaggerating the movement of a rolling wire of exceedingly small diameter, placed between two moving planes or plates, is certainly one of the most ingenious methods of exhibiting extension that could possibly be devised.

Mr. Stromeyer : My Lord and Gentlemen, I thank you very much for the way in which this Paper has been received, The Paper is not what I had intended it to be, viz., a detailed analysis of the actual strains in a ship as they occur at sea, but as I see no immediate prospect of carrying out such experiments, and as the instrument is now in perfect working order, I have brought it forward on this occasion in the hope that some one with more leisure or better opportunities would employ it to such a purpose. The Paper contains almost all the necessary instructions for such work, though I have no doubt that I would be able to supplement them by various useful hints. The instruments which I have exhibited to-day are both in perfect working order; but as they are the first ones that have been made, and as they have been altered several times to improve them, they have the appearance of being imperfect. I have not shown the mechanism for moving the metallic paper or blackened glass, as you will readily understand that they present no great difficulties. The diagrams which are contained in the Paper are the best proof of this. In answer to Mr. Biles' question about the launching strains, I think it will be the best plan to treat the launching strain Fig. 5 in greater detail, because, being a diagram, it is the most complete record of all the four launches. It will be seen that the curve rises very considerably when the vessel first commences to move; this may be due to the ways not being of a uniform curvature; the strain rose in fact so much that the pointer left the paper and only came back to it while the forefoot of the cradle was just leaving the ways. At this point the intensity of the stress was about ·4 tons and it subsequently rose to ·5 tons as the vessel came to rest. The marked irregularities in this part of the curve are no doubt due to the slacking out of the holding cables. Thus the difference between the stress in the ship's side, before and after launching, is about half a ton. What the initial strains are cannot, of course, be known because the distribution of the pressure of the ways cannot be known. But this is comparatively immaterial, as I have tried to show elsewhere. With but one diagram to work with, it would be useless to base any conclusions on this experiment, but the fact remains that strain diagrams can be

taken with the instrument. The cases of the S. S. "Hooper" and the S. S. "Compton" are more instructive, and if I had not been afraid of overcrowding the Paper, I would have sketched the distribution of the longitudinal strains in an empty ship at sea. All the necessary information for doing this is contained in Table VIII. In conclusion, I wish once more to thank you for the kind attention I have received.

The PRESIDENT: I am sure you will allow me to thank Mr. Stromeyer on your behalf for the valuable instrument that he has exhibited, and the clear explanation which he has given of it. I need not remind you that you have had two most important discussions on two very valuable indicators, and when we consider the amount of research and observation and calculation necessary to bring these instruments to their present apparent perfection, our thanks are doubly and trebly due to those who have expended so much time in bringing the result of their scientific investigations and calculations into a simple practical form, within reach of the use of our captains, for the purposes of navigation. I really think, Gentlemen, the thanks of the community at large are due to gentlemen who devote large portions of their time and labour to the perfecting of these most valuable indicators, and I venture to thank, on your behalf, those gentlemen who have contributed so much to the public service.

A BRIEF REVIEW OF THE PROGRESS OF MILD STEEL AND THE RESULTS OF EIGHT YEARS' EXPERIENCE OF ITS USE FOR SHIPBUILDING PURPOSES.

By B. Martell, Esq., Member of Council.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 15th, 1886 ; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

Previous to the year 1875 Mild Steel, as we now designate the material, was practically unknown in the construction of merchant vessels.

In that year Mr. Barnaby (late Director of Naval Construction) read a paper before this Institution, in which he described some Bessemer and Siemens-Martin's steel which he had seen used for plates and beams in war vessels building at the French Naval Establishment at L'Orient.

This steel was required, by the French Admiralty, to have a tensile strength of $30\frac{1}{2}$ to $31\frac{1}{2}$ tons per square inch, with a ductility represented by an ultimate extension of 22 per cent. The angle and beam bars were curved and bevelled by pressure, the use of iron hammers for bending the bars to the required form being scrupulously avoided.

One of the French Authorities, referring to the manipulation of the steel, pointed out the care and skill necessary to avoid rupture in plates, where hammering or severe local pressures were unavoidable, or considerable curvature was required, and he remarked that light blows should be delivered over as large a surface as possible, and the curvature produced by successive stages, and after the work was done the plates should be promptly annealed.

Mr. Barnaby stated that he thought a material which needed such care in its treatment would stand a poor chance in an ordinary shipyard, and that he should feel very doubtful about a ship built of it, unless he could see every plate worked. In concluding his paper, he alluded to the uncertainties and treacheries of Bessemer steel in the form of ship plates, requiring all the care which had been bestowed upon it at L'Orient to avoid failure, and added, "We want a perfectly coherent and definitely carburized bloom or

ingot of which the rolls have only to alter the form in order to make plates with qualities as regular as copper, and we look to the manufacturers for it."

This now famous challenge was replied to in a Paper read at the meeting of this Institution in the following year, by Mr. Riley, then Manager of the Siemens Steel Works at Landore, in which it was shown that mild steel could be manufactured at the Landore Works of uniformly ductile quality. At the end of his interesting paper, Mr. Riley, after enumerating the advantages claimed for Siemens-Martin's steel as manufactured by his firm, over iron, stated that these considerations forced upon him the conclusion that only a short time must elapse before the value of the material being more and more recognised, it would replace iron in the construction of vessels.

In the course of the discussion which ensued on that Paper, I remarked that we appeared to have arrived at a point where we had a material such as we could use with confidence for shipbuilding purposes—a material in which we could not only obtain uniformity, but the very great advantage of the temper not being very materially affected by punching and shearing, a very important point, for if all the plates required annealing after punching, we could not feel confidence in its use for mercantile shipbuilding; and this annealing of all plates could not be done in a merchant shipyard. The question of cost was, of course, of great importance; and the relative sizes for steel as compared with iron which could be admitted would require serious consideration. My belief, which I then expressed, was that if such a material as this could be produced at a reasonable price, it would be very considerably adopted for the building of mercantile vessels.

Early in the following year (March, 1877) plans were received at Lloyd's Register from Messrs. John Elder & Co., for the construction of two paddle steamers of the following dimensions—221·3 by 27·7 by 10·6, which were proposed to be built of Siemens-Martin steel, and to be employed in the English Channel passenger service. In these vessels the scantlings were approved by Lloyd's Register Committee at a reduction of some 20 per cent. from what had been adopted for iron vessels for similar service. These vessels, I have reason to believe, have answered well in every way, and have fully borne out in their behaviour the anticipations expressed as to the suitability and superiority of mild steel over iron in the construction of merchant vessels.

In the same year, in company with some of my colleagues, I visited the principal steel works in the country for the purpose of making a report on the question of the conditions under which mild steel might be sanctioned in the general construction of merchant vessels to be classed in Lloyd's register book for general purposes of trade, and a copy of that report

is appended to the Volume of Transactions of this Institution for 1877. In that report it was recommended that steel should be admitted in the building of ships for classification of scantlings generally 20 per cent. lighter than prescribed for iron, subject to the material fulfilling certain defined tests, viz., a tensile strength of 26 to 30 tons to the square inch, and an elongation of 20 per cent. on a length of eight inches. These proposals were approved by Lloyd's Register Committee, and rules to this effect were issued to the public. Subsequently the tensile limits were slightly increased to 27 to 31 tons to the square inch, as being in accordance with further experience gained of the material in its use in the mercantile marine.

Proposals for building steel sailing and steam vessels rapidly followed the issue of these rules, and resulted in the construction of seven steamers of an aggregate tonnage of 4,490 tons, which were classed in Lloyd's Register in the year 1878, whilst in addition a number of steel vessels were in course of construction but not completed in that year.

At the meeting of this Institution in that year, I had the honour to read a Paper on "Steel for Shipbuilding," in which were included the results of a large series of experiments which the Committee of Lloyd's Register had caused to be made under the inspection of their officers.

Those experiments showed conclusively in my opinion the great superiority of mild steel over iron for shipbuilding purposes. I had, before preparing that Paper, made careful personal enquiries of many engaged in the manufacture and the use of the new material in shipbuilding, as well as those whose duty it had been to officially inspect the working of it, and I was able to elicit but one opinion, and to which I can bear my personal testimony, that within certain limits of thickness, this mild steel—as had been described by Mr. Riley two years before in his Paper—was a material which could be used with the greatest confidence and reliability for shipbuilding purposes.

I am most firmly of that opinion now, after eight years' extended experience of its use.

Doubtless there are peculiarities in the nature of this material which are not found in iron, and recent experiments are enabling us to understand these more clearly, particularly the changes which occur in its properties of ductility and elasticity under changes of temperature, but, so far, nothing has been discovered which would show that as regards working the material, if ordinary intelligence be used, the usual modes of manipulation may not be adopted as in the case of iron.

The question of cost was to my mind one of the most serious impediments to the advance in the tonnage of steel vessels, and in the paper to which I referred, I took the

opportunity to draw particular attention to this, and to this additional cost was doubtless due the fact that very slight addition was made in steel vessels the following year, 1879.

It could not be expected that shipowners generally would be governed in their selection of the material by any purely sentimental appreciation of the superiority of steel, without regard to the question as to whether a steel vessel would be more costly to build, or more expensive to manage in times of keen competition.

Moreover, the number of shipowners who would be likely to have their vessels built of a comparatively untried material simply in the hope that they would be stronger, or be less liable to loss from collision, grounding, &c., would probably be very small, unless it could be shown that an equally fair profit might be looked for from the additional outlay.

I consequently endeavoured to show in the paper above mentioned that a steel built vessel would be rather more economical than the iron one, if properly designed and managed.

To the causes I have mentioned, especially that of first cost and the prejudice against novelty, which stood against the rapid introduction of steel vessels, must, of course, be associated the opposition that might have been expected from the enormous interests vested in the ownership of iron vessels and in the manufacture of ship iron, and it was not likely that the new material would be allowed to make headway without many obstacles being placed in the way.

Again, it could not be hoped that any considerable advance would be made before some experience had been gained as to the behaviour of the vessels recently built. The question of corrosion—more important in a steel vessel on account of the decreased scantlings—was not by any means settled. Probably the doubt on this point had as much or greater effect than any other consideration in deterring many from embarking in the building of steel vessels.

In any case there were only eight steel steamers of 14,300 tons, and one sailing vessel of 1,700 tons built and classed in Lloyd's Register in 1879, as against seven steamers of 4,470 tons built and classed in the previous year. It will, however, be observed that the average tonnage of vessels built of steel in the latter year greatly exceed that of the vessels built in the preceding year.

An important cause of discouragement, as affecting the use of steel in shipbuilding, was brought under notice by Mr. W. Denny in a valuable paper read by him before this Institution in 1880, in which strong objection was taken to the system of testing the

material at the shipbuilding yards adopted by Lloyd's surveyors, by which great delay and unnecessary cost resulted in cases of failures. My own personal views were in favour of as little restriction being imposed to the use of mild steel as was compatible with effective testing and detection of defective material, and I was able to add, in some remarks that I made, that the Committee of Lloyd's Register had the subject of testing at the manufacturer's works, so strongly advocated by shipbuilders, at the time under consideration. This desire of the shipbuilders was shortly afterwards sanctioned by Lloyd's Committee.

The appointment of surveyors at the several important steel works, for the purpose of testing the material before leaving the premises, has resulted in giving satisfaction to the makers as well as the shipbuilders, and the uncertainty as to the suitability of the material, and the delays due to rejections at the shipyards, no longer stand in the way of the use of steel, to which I am confident the special appointment of inspectors for testing at the works of the makers has given no slight impetus.

In the year 1880, the number of steel vessels classed in Lloyd's Register, increased to 21 steamers of an aggregate tonnage of 34,031 tons, and two sailing vessels of 1,342 tons; whilst the iron vessels—steam and sailing—had only increased to 355 as against 348 in the previous year. Thus in 1880 the building of steel vessels showed decided progress.

In 1881 there were 20 steel steamers and three steel sailing vessels classed at Lloyd's, whilst the number of iron vessels was 452.

In the year 1882 following, 61 steel vessels, of a gross tonnage of 125,841 tons, were classed at Lloyd's, as against 525 iron vessels, of an aggregate tonnage of 851,075 tons; being no less than 10 per cent. of the entire number of iron and steel vessels.

Coming to the year 1883, we find the unparalleled amount of 1,100,000 tons of iron and steel shipping was built under the survey of Lloyd's surveyors and classed in their Register. This comprised no less than 109 steel vessels, viz., 94 steamers of 150,725 tons, and 15 sailing vessels of 15,703 tons—being about 14 per cent. of the total tonnage.

A rapid decline took place in the amount of shipping turned out in the United Kingdom in 1884; the total classed at Lloyd's of iron and steel ships being 607, of an aggregate tonnage of 793,658 tons. Of these 92 were steel of 132,457 tons—exceeding 16 per cent. of the total tonnage of iron and steel vessels combined.

During the year ending December, 1885, a great stride was made in the output of steel vessels; as we find that out of a total of 378 vessels built for classification at Lloyd's, no fewer

than 118 were of steel, or nearly 30 per cent. of the whole, while the tonnage in steel was over 36 per cent. of the gross tonnage of iron and steel vessels turned out. So that while the iron ships had declined 100 per cent. from the number in the preceding year, the steel vessels had increased about 25 per cent. in the same time.

Such then has been the progress made in the use of mild steel for shipbuilding purposes in so far as the returns of vessels classed in Lloyd's Register are a criterion. And I venture to think that as about 90 per cent. of the whole shipping of the country have for the last few years been built for classification at Lloyd's, the figures I have given afford a fair indication of the relative proportions of steel and iron vessels either in existence or constructed within the dates given.

The following table shows the number and tonnage of steel and iron, steam and sailing ships which have been built to class at Lloyd's Register since the introduction of mild steel for shipbuilding.

STATEMENT OF THE NUMBER AND TONNAGE OF STEEL AND IRON STEAM AND SAILING VESSELS BUILT AND CLASSED DURING THE SEVERAL YEARS COMMENCING 1878.

YEAR.	Steel.				Iron.				Total (Steam and Sailing).			
	Steam.		Sailing.		Steam.		Sailing.		Steel.		Iron.	
	No.	Tonnage.	No.	Tonnage.	No.	Tonnage.	No.	Tonnage.	No.	Tonnage.	No.	Tonnage.
1878	7	4,470	—	—	329	406,196	106	111,496	7	4,470	435	517,692
1879	8	14,300	1	1,700	318	436,339	30	34,630	9	16,000	348	470,969
1880	21	34,031	2	1,342	324	422,622	31	37,372	23	35,373	355	459,994
1881	20	39,240	3	3,167	401	622,440	51	74,284	23	42,407	452	696,724
1882	55	113,364	8	12,477	457	742,244	68	108,831	63	125,841	525	851,075
1883	94	150,725	15	15,703	576	817,584	68	116,190	109	166,428	644	933,774
1884	79	120,081	13	12,376	417	517,459	98	143,742	92	132,457	515	661,201
1885	89	129,824	29	35,613	142	141,343	118	149,086	118	165,437	260	290,429

These numbers include steel and iron ships built on the Continent, in America and the Colonies.

Now as to the experience of the behaviour of steel vessels in actual work at sea, and as to the proportion of losses.

The only total losses that I have been able to ascertain as having occurred to steel vessels are seven in number.

These are—1st. The SS. "Aristides," of 1,536 tons, built at Liverpool in 1881, and lost by stranding in December of the same year.

2nd. SS. "Chiapas," of 1,582 tons, built at Glasgow in 1882, and lost in the Irish Channel on her first voyage. There was no evidence to show how the water found its way into the vessel.

3rd. SS. "Seraglio," of 1,816 tons. Foundered in a cyclone near Aden.

4th. SS. "Magneta," of 922 tons, built at Glasgow in 1884, and lost off the coast of Spain.

5th. SS. "Dartmouth," of 367 tons. Supposed to have foundered during a heavy gale which sprung up immediately after leaving port.

6th. Twin SS. "Notting Hill," of 4,021 tons, built at Glasgow in 1881, and sunk after a collision with an iceberg.

7th. Twin SS. "Holyhead," of 841 tons. Lost by collision with the sailing vessel "Alhambra" in the Irish Channel.

From this will be seen that, considering these losses extend over eight years, the immunity of steel vessels from *total* loss appears to be remarkable.

We have recently had under consideration at Lloyd's Register Office the question of the strength and durability of steel vessels, and, in order to obtain as full information as possible, the whole of the members of the surveying staff were requested to furnish the results of their observations in connection with the actual inspection of steel vessels which had come under their examination after having been some time engaged in ordinary trade at sea.

In all, about sixty vessels were specially reported upon, ranging in size from vessels of 220 tons to above 5,000 tons, and from seven years of age down to those of most recent construction.

The results of their experience and information as regards the strength and efficiency of vessels built of mild steel to the requirements of Lloyd's Rules, and as to the effect of corrosion, are embodied in the following illustrations and general conclusions :—

1st. The case of the SS. "Rotomahana," of 1,727 tons, built by Messrs. W. Denny and Bros., is too well known to need recapitulation, and will be found in an interesting Paper read before this Institution by Mr. W. Denny in 1880.

2nd. The SS. "Minard Castle," built in 1880, and steel plated, went ashore, and was sharply indented in the bottom, but little or no leakage resulted, although the iron floors and frames, where the bottom was indented, were crushed or broken.

3rd. The SS. "Warwick," of 2,527 tons, built in 1882, was damaged by grounding in the River St. Lawrence. She was got off, temporarily repaired, and sent across the Atlantic to be permanently repaired in this country. The surveyors reported that the whole of the external bottom was crushed in, but there were no symptoms of straining in the butts of the outside plating above the bilges. They added that the damage was of such a serious character in the bottom, that had the vessel been of iron she must have gone to pieces.

4th. The SS. "Fawn" struck the piers at Caen. The opinion of those who saw her was, that had the vessel been built of iron she must inevitably have sunk.

5th. SS. "Duke of Westminster," of 3,726 tons, went on shore, when loaded, off the Isle of Wight, and was beating on the ground during heavy weather for some days. She was ultimately got off, brought to London, and was placed in dry dock. On examination the plates in the bottom, nearly all fore and aft, were found to be indented between the frames, and had to be removed. It is needless to say, that iron plating under similar circumstances would have been broken.

6th. SS. "India," of 2,634 tons, grounded whilst loaded. She was got off without any leakage, and on reaching the port, was placed in dry dock, when it was found that the keel and bottom were set up to an extent of about five inches, but no part was broken.

7th. SS. "Austral," of 5,589 tons. This vessel, whilst lying at anchor in the Clyde, was run into by the ss. "Cassia," of 1,163 tons, going at a speed of 5 to 6 knots. The blow was delivered amidships, and deeply indented a number of plates below the water line, but none of them were broken, whilst the damage to the bow of the striking vessel was extremely severe. It was the opinion of those on board, that had she been an iron vessel, the side must undoubtedly have been crushed in, and the vessel sunk. This case, in view of the recent lamentable accident, is perhaps more important than any of the foregoing cases.

8th. SS. "Takapuna," of 930 tons, built at Barrow in 1883. This steamer struck a rock when going full speed, and the Captain had omitted to enter it in the log book; and as

the ship did not leak, no notice was taken of it. It was, however, afterwards found to be noted in the engineer's log, and on being placed in dry dock for examination, it was found that the concussion was of that nature that it must have sunk an iron vessel.

The above are only typical illustrations of cases which have come to the knowledge or under the observation of myself and officers of Lloyd's Society, practically showing the great superiority of mild steel, as compared with iron, in withstanding concussions from collisions and grounding.

The question of corrosion is one of considerable importance as affecting steel vessels, particularly, as before stated, in view of the reduction in scantlings as compared with vessels built of iron. Constant attention has consequently been given to this subject by the Surveyors to Lloyd's Register, as the vessels have from time to time come under examination. The general opinion expressed by them is that so far as corrosion is concerned, mild steel is not more injuriously affected by oxidation than iron, provided the mill scale be first cleaned off the surfaces before coating the steel. In cases where this precaution had not been taken rapid deterioration was found to have taken place. The removal of this black oxide or mill scale is therefore most important, and was illustrated by some experiments which had been made by the Admiralty and explained in a Paper read before this Institution by Mr. Farquharson in the year 1882. The results of these experiments have been fully borne out in the practical experience acquired in the survey of steel vessels. If, however, the scale is rusted off before painting, and the coating of plates be carefully done, no serious corrosion need be apprehended.

There is no doubt the most effectual mode of cleansing the plates of this scale is that adopted by the Admiralty, and by some few private shipbuilders, viz., immersing the plates in a bath of diluted hydrochloric acid.

In order to preserve the surfaces of thin plates from corrosion, Messrs. W. Denny and Brothers have resorted to the system of galvanizing the plates, and for vessels intended for river purposes, where thin plating is essential, this mode is thought to be very effectual.

I would, however, strongly urge that every steel vessel should be placed in dry dock for the examination of the bottom within a period, say six months, after launching.

It may be added that vessels have been specially surveyed after four years' service, in which nothing was observed to call for any special remarks as the result of having been

constructed of steel, whilst steel cargo vessels which have been in constant employment during the last six or seven years have on recent examination been found free from pitting or any undue corrosion.

Another point of some importance—particularly as affecting steel vessels which have received damage by indentation and alteration of the form of plates—is that of local heating for the purpose of restoring plates to their original form. It is held by some who have experience in the working of steel, that this can be done without detriment to the original strength of the material. A case bearing on this has come under my observation within the last few days. A large steamer while lying by the side of a wharf at New York was subjected to intense heat from a fire which had arisen on the wharf.

The starboard side plating from the weather deck to the water's edge was exposed to the fire, and the deck was burnt nearly all fore and aft.

After the fire had been got under it was found that the side plating was more or less distorted between the frames, and some of the upper plates were cracked, and the butts drawn apart by the contraction in sudden cooling. Abreast the mainmast the main sheerstrake and strake below were broken right across the plates. The fractured plates were either renewed or strengthened by outside doubling straps for the voyage home.

The vessel left New York for England, and all went well for two days, but on the morning of the third day—early in the morning—the weather being very boisterous and the ship rolling heavily, a loud report was heard, which—as may be expected—produced considerable agitation amongst those on board. On a careful examination when it became sufficiently light, it was found that on the side of the vessel upon which the fire had played, the main sheerstrake at some distance abaft the original fracture, and two strakes of plating immediatety below were completely fractured across, the sheerstrake being broken along the outer column of riveting of the buttstrap, and the butt joint itself being considerably opened; the fracture ended at the third strake.

On the next day, another loud report was heard and it was found subsequently that at a distance of about 24 feet on the after side of the fracture I have described, the sheerstrake was considerably drawn at the butt; the plate above and its doubling—for the strake above the sheerstrake was doubled—were both broken entirely through, the distance between the two parts of the fracture being nearly one-eighth of an inch.

The plate below the sheerstrake was found to be cracked to a very slight extent.

This vessel is now under special survey by the London surveyors to Lloyd's Register, and a great number of plates will have to be renewed. I hope to gain further data on this important subject, after portions of the broken plates have been tested, particularly in the vicinity of the fractured parts.

As regards the structural strength of steel vessels; if they be faithfully built to the scantlings admitted by the rules of Lloyd's, there is no reason to apprehend that they are not sufficiently strong, even with the diminished scantlings, to perform all that is required of them in a satisfactory manner. After a careful examination of a large number of steel vessels classed at Lloyd's, which have from time to time during the last eight years come under my observation, I am strongly of opinion that if vessels are built of this material according to the present requirements, and the workmanship be faithfully executed, and proper attention paid to preserving the continuity of strength, they are capable of performing their work with the greatest efficiency and satisfaction.

At the same time I am firmly convinced of the greater safety of steel ships, especially in cases of grounding or collision, and although steel vessels have in some instances required heavy repairs, yet, owing to the superior ductility of the material, they have been recovered from accidents which would have entailed loss of life as well as of property if they had been built of iron.

In recently reporting to the Committee of Lloyd's Register on the whole subject, I felt it proper to say that, so far as our experience has gone, "I am of opinion that the total losses of steel vessels as compared with iron will be less, but comparatively very extensive repairs may be looked for, as owing to the greater ductility of mild steel, many more vessels which go on shore will ultimately be got off with, in many cases, considerable distortion of form, involving greater repairs than have usually been the case with vessels built of iron."

I desire to take advantage of this opportunity to reiterate the opinion which I had not hastily formed in 1877, that mild steel would, under proper supervision, at no distant date supersede iron as the material for shipbuilding, and also further to assure those who, from what has been brought under my notice, appear to have drawn the conclusion that my views, as regards this, have considerably changed, that I entertain the same opinion as steadfastly as ever.

I think it may be of interest to some if I avail myself of this opportunity to explain my views on steel ships *versus* iron ships, not only from a professional but also from a

commercial point of view, in order that the minds of the owners of steel vessels and those interested in their construction should not be disturbed by any opinions detrimental to them which I am supposed by some to entertain.

It must be evident that in the case of two similar vessels in all respects, the one built of iron and the other of steel, if they were both subjected to the same strains, sufficient in the case of the iron vessel to break her asunder, that the steel vessel, though constructed of a stronger and more ductile material, must, if the strain be continued, either break or change her form. In a large number of cases I think it will be found that in cases of grounding, or being subjected to similar accidents to which we know ships are liable, that steel vessels will not break, but that many will become greatly distorted by such strains, whilst continuing to hold together, so that they can be raised or got off, and be brought under survey for repairs. In such cases the question arises :—

1st. How would the insurers be affected by such possible contingencies, supposing the iron and steel vessels to be insured at the same premium ?

2nd. What is likely to be the character of the repairs required, having in view the just claims of the shipowner as against the insurers, viz., that his vessel should be put into practically the same condition as before the damage ?

It appears to me that, as regards the first consideration, the owner of the steel vessel could not be put in a worse position than the owner of an iron vessel which under similar circumstances had become a total wreck. Because, if it can be shown that the cost of repairs exceeds the stipulated proportion of the total value, he would have a legal claim to be paid the full insured value as a constructive total loss. On the other hand, the insurers, though paying the full value insured, would still retain the vessel as salvage, which could not be so with the iron vessel which had broken up.

Now, notwithstanding the steel vessel, without being practically rebuilt, could not be placed in the same condition as before the damage, still there would arise many ways by which she could be repaired and strengthened, and be made a vessel of considerable commercial value for many purposes of trade.

Under these circumstances I fail to see, as some suggest, that such an occurrence would place a steel vessel, as compared with a similar vessel built of iron, at great disadvantage, either as regards the shipowner or insurers. In fact, it appears to me that the advantage would be on the side of the steel vessel.

Secondly, as to the character of the repairs which may become necessary.

I think the proper course in dealing with the question of necessary repairs to a classed vessel which has to retain her class, is to regard the case from the point of view of a surveyor who has to become responsible for the efficient performance of the vessel after the repairs have been completed. It is urged by some that in cases where vessels have become seriously strained, it may be taken for granted that the structure is sufficiently sound and undisturbed at the butts of the outside plating, if it becomes impossible to insert a testing knife in the butts. This I entirely dissent from, and I am sure I shall be borne out by many practical surveyors that iron or steel vessels may be very considerably strained and become unseaworthy when loaded, from leakage at the butts of the outside plating, although it would be impossible when the vessel was on blocks to insert the thinnest blade of a knife in the butts of the plating. I will only instance one case in support of this view. I was called on, in conjunction with some of my colleagues, to survey a large steamer which had nearly foundered in consequence of leakage through the outside. When the ship was in dry dock, a most careful survey was made of all the butts, but it was impossible to detect any opening of the butts, neither could any insertion whatever with a thin knife-blade be made. Those who sailed the ship pointed out to us on the inside where water had poured in at the sides of the butt straps in a fan shape sufficient to endanger the safety of the ship when she was labouring and straining in a heavy sea. This vessel had to be strengthened at a cost of more than £10,000 before she was again sent to sea, after which no leakage occurred, showing conclusively that the water found its way through the butts, though nothing indicating the same externally could be seen. Many cases of this kind are known to practical surveyors. Such being the result of experience, I am of opinion that in such cases as I am referring to, where a vessel has been severely strained to such an extent as to produce distortion of form, the straining at the butts would require the refitting of the butt straps and consequent re-riveting before a surveyor could feel justified from mere local external appearance in assuming that the connection of the butts was uninjured.

Next, assuming it to be the case that from violent concussions against an immovable structure one side of a vessel had to a considerable extent been driven in, and the opposite side set out, and the vessel otherwise distorted from this and subsequent causes. Should the owner decline to compromise with his insurers, and claim to have his vessel put practically into the same condition as before the damage, it is difficult to conceive how the necessary repairs could be effected without a practical reconstruction. Doubling strakes might be suggested, which would doubtless render the vessel as strong as before the accident, provided the soundness of the riveting were first assured; but it must be borne in mind that

the owner has to be reckoned with in such a case, and if the vessel be nearly new, many owners would have a decided objection to having a portion of the vessel doubled, fearing it might detract from the vessel's marketable value, as well as making her more unprofitable in consequence of the additional weight of the hull.

Again, as to the damage the steel might have sustained from severe straining. A surveyor having the responsibility of the efficiency of the vessel after completion, could not be certain that such a ductile material had not been strained in such a case beyond its limit of elasticity, and would therefore not be justified in passing such strained material unless he had an opportunity of testing some of the plates to assure himself of their condition. Should the tests show that the plating had not been unduly strained to an extent to alter its character, no doubt the plates throughout the uninjured part might remain on, subject to being re-riveted. The plates also, in the damaged portion, if found equally satisfactory, might be re-rolled and replaced, but many of these, particularly in the curved portion, would doubtless, as experience has shown, be found altered in form to such an extent as to prevent sound riveting being made, and would have to be condemned.

Such in my opinion are the general views which would be held by a surveyor responsible for the efficient repair of a vessel in the damaged condition assumed, and it is difficult to understand how these views greatly differ from those which would be held were the vessel constructed of iron instead of steel, and she had suffered similar damage without breaking, though altered in form.

I have endeavoured to explain my views frankly, bearing on the general subject of iron vessels *versus* steel vessels, but have striven to avoid importing references to special circumstances which may possibly be in the minds of some. This Institution is, in my opinion, not a place where questions of a personal nature should be discussed, or a place which I feel I could select, in which to justify any professional opinions expressed by me elsewhere, and perhaps after what I have said here as affecting the general question of steel ships any further explanation may be considered unnecessary. If it is thought that some personal references to myself throughout this paper might have been avoided, I can only apologise and claim the indulgence of the audience under the circumstances. I have found it almost impossible to avoid these references in view of the subject of the introduction of mild steel in the mercantile marine being so closely associated with the action which Lloyd's Register Committee have taken from time to time in relation to this material, and my official connection with the Committee. This I trust will be taken as an excuse, if any be needed, for the references to which I have alluded.

I cannot conclude without a passing reference to recent experience as to the suitableness of steel produced by the Basic process for shipbuilding purposes. There is no doubt that mild steel is being produced by this process of a reliable quality at a comparatively low tensile strength, and well suited for many purposes to which it can be applied. But experience has abundantly proved that up to the present time Basic steel has not been produced, possessing those uniformly ductile and reliable quantities at high tensile strength necessary for shipbuilding purposes. Some time ago some vessels were being built in Germany under the survey of Lloyd's surveyors to class in their Register, and owing to so many failures which were occurring in the material I was instructed to visit the Continent to investigate the cause of these failures. I ascertained that the steel being used was made in Germany by the Basic process, and on testing a large quantity of it I found it very unreliable and unsuitable for the purpose. The manufacturers after testing a number of vessels' frames which they had turned at their own works, admitted its unreliabity, and gave me a written guarantee that no steel in future made by the Basic process should be supplied for these vessels, as they were unable with this material to fulfil the conditions laid down by Lloyd's Register Committee.

Very recently striking evidence has been adduced in this country that those who undertook to supply steel for vessels to class in Lloyd's Register have been unable to fulfil the conditions imposed, with steel made by the Basic process, and have had to relinquish the attempt. Under these circumstances the Committee of Lloyd's Register have felt compelled to decline for the present to accept steel made by the Basic process for vessels intended for classification in their Register book.

Such great advances however have been made in recent years in perfecting the quality of mild steel, and such perfectly reliable and uniform material is now being made by the Siemens-Martin's process, that it can only be hoped similar success will ultimately attend that produced by the Basic process for shipbuilding purposes; and that the surviving inventor of the system by which it is made, will live to reap the full reward of the labour and intelligence which have already been brought to bear upon its production.

THE PRESENT ASPECT OF MILD STEEL FOR SHIPBUILDING.

By J. Ward, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 15th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

There are few industries in our country which, through scientific skill, have undergone such marvellous changes in so few years as the professions of shipbuilding, bridge building, and boiler making through the successful manufacture of mild steel by the process known as the Siemens-Martin.

So many able members of our Institution have at previous annual meetings recorded their experience of this material and furnished data for general guidance that it becomes a somewhat difficult matter to say anything absolutely new or fresh on the subject.

The present aspect of mild steel as applied to shipbuilding is, however, specially interesting from the fact that the relative prices of steel and iron so closely approach each other that steel steamers can now be built on the Clyde as cheaply as iron ones of the same dimensions, thus giving the owner of a steel steamer the advantages of greater dead-weight, and a material which is much more reliable under all conditions of the ship's life than iron.

This being so, and the firm of which I am a member having had, perhaps, as large an experience in the use of this material as any of the professional members of our Institution, it may neither be uninteresting nor uninstructive if I supplement the Paper read before this Institution by my partner, Mr. William Denny, in 1880, and put on record a general statement of the work done by us in this material up to date, of the treatment given it in working, of total losses through failures in working, and, generally, the reasons our clients and ourselves have for our faith in this material under almost all conditions of treatment.

Since eight years ago we built the Union Company of New Zealand steamer "Rotomahana," the first ocean-going merchant steamer ever built in this material, we have

delivered to owners 80 steel vessels, varying in gross tonnage from 50 to 5,000 tons, and aggregating upwards of 118,000 tons gross register. We have used in their construction upwards of 51,000 tons of mild steel, made up of nearly 375,000 separate pieces of plates, angles, channels, bulbs, and bulb tees. This number is made up from the order books in which lugs and other small pieces are ordered in long lengths. If the work at present on hand in the yard is added, our tonnage and total of material used would be proportionately increased.

In working this material we have for the last five years gone on the general principle that if steel could not stand the maximum amount of rough usage it is sure to get in a shipyard in punching, shearing, hammering, furnacing, &c., the sooner it failed and the defects were made known and remedied the better; for, were special precautions necessary for working it, as many people still believe, so surely would this necessity bring about its certain failure and rejection, as no amount of instructions would ever gain it better or different treatment at the hands of the workmen than iron has always had.

In this view the workman would be right, as no material striving to supplant iron should need or receive special favouring.

The ability of steel to stand successfully all conditions of rough usage in working is an important and necessary evidence of its suitability to resist the severe local and general strains certain to come upon it when worked into the vessel's hull.

Our total failures in working Siemens-Martin steel are divisible into two periods:— 1st, that up to 1880 embodied in Mr. Wm. Denny's Paper of that session; and 2nd, since 1880 till now. The failures are shown on diagrams (Plates VIII. and IX.) and may be summarised as follows:

In the first 7,000 tons used, embracing 58,000 separate pieces of material, six plates and one angle bar failed.

In the last 48,000 tons, embracing work to date and consisting of about 350,000 pieces, seven plates, two angle bars and three bulb tees failed.

We have often lost more than four times this amount in a single iron vessel.

These results under treatment, such as I have named, are worthy of as much recognition by authorities interested in steel as the occasional failures receive, and go

to prove that under conditions of local heating, or even blue (called in shipyards black) heat, failures in shipyards of Siemens-Martin steel are the rare exceptions and not the rule.

Local heating, without subsequent annealing, is generally condemned by most authorities on steel, including Lloyd's Register. This condemnation is at variance with the practice allowed by the latter body. In private shipyards, boiler shops, and Admiralty dockyards, local heating is largely carried on. In fact, it is almost impossible to build a vessel without a considerable amount of local heating in some sections of the work—notably all the deck beams to which knees are welded, or the ends of the plates in all water-tight bulkheads, which are drawn out at the corners to admit of their fitting between double frames.

In Admiralty vessels, where the amount of water-tight joggled work is very large, it is all done under the effects of local heating. In shipyards the local heating of shell or deck plates is not usually necessary, but in boss, oxter and bridge plates it is not at all an uncommon thing, and with no apparent detrimental results.

Local heating in the construction of marine steel boilers is quite a common practice in the Clyde district, some hundreds of furnace and combustion chambers having been made by welding, and no difficulty whatever experienced with the material. Local heating by welding in this manner is found to be quite satisfactory, and even without annealing is considered an improvement over lap joints and seams of rivets, or butt straps. A more severe test of probable damage from local heating often occurs in making large steel boilers where it is almost impossible to get the plates perfectly fitted for riveting, save by local heating and hammering close—one of the instances being where circumferential plates cross the joints of boiler fronts.

Neither is it at all an uncommon occurrence the lifting of furnace crowns, which after a long voyage have, through carelessness or other cause, come down. Not only are they locally heated, but severe local strains are set up owing to the lifting of the crown being done by means of a screw jack localised where the heating has taken place. Boilers used in this way seem none the worse for the treatment.

Blue Heat.—Fracture at a blue heat is known as a reality, and from the valuable tests of Mr. Barnaby some years ago, as also Mr. Stromeyer's Paper recently read at the Institution of Civil Engineers, one would expect to find it of common occurrence in the

shipyard. As a matter of fact this is not the case, and so far as our experience goes is very rare. In the 80 vessels mentioned, we have heated and bent between 35,000 and 40,000 frames and reverse frames for them, all of which have been worked during the temperatures of red to blue heat, and in almost all cases finished at a blue heat. Save in the cases shown on diagrams, we have never lost an angle bar when so working it.

Keel and ballast tank wing plates are flanged and worked at heats starting with red and finishing with blue heat. Boss, oxter, and bridge plates, also forward and aft plates in vessels with plate keels have often to be furnaced twice and even thrice before proper set is got. These always get a large amount of rough usage from workmen by hammering till cold and yet with marvellously few failures.

In carrying out recently the following experiments, 1, 2, 3, Table I. (*see* Plate X.), which speak for themselves, the object was to test the working of ordinary steel under conditions fully equal to those involved in repairing a badly damaged vessel and under the before mentioned conditions of local and blue heating, &c. The three experiments shewn on Table I. were made in our own works, from ordinary steel plates taken from stock, and the dynamite and temper tests shewn on Tables II., III. and IV., were made by permission of Mr. James Riley, of the Steel Company of Scotland, at their Hallside Works from ordinary ship plates of their manufacture.

These dynamite tests are both interesting, exhaustive and instructive, but their severity is somewhat unnatural, and is greater than could possibly occur to a damaged steamer. The plates were only 2 ft. 6 in. square, and the damage was consequently concentrated on one portion of a small area, while in the 12 to 16 ft. lengths of ordinary shell plating the damage could hardly be localised to the same extent. The wonder is that plates could successfully stand such treatment, and while the tests show the comparative results of same under conditions of cold, red and blue heats, yet the accompanying tests from actual damaged plates taken from a steel steamer and treated hot and cold (*see* Tables V., VI. and VII., and Plates XI., XII., XIII. and XIV.) are of more value as affording reliable data for guidance.

It should not be forgotten that, even after the dynamite and blue heat treatment, the steel in the immediate vicinity of the dishings is still better than average iron supplied to shipyards (*see* Table III.) while tests taken from portions clear of the dishing which received a considerable amount of work at this heat are quite satisfactory in respect to strength, ductility and bending (*see* Table II.).

The record of tests made last year, and shown in Tables V., VI. and VII., from four damaged shell plates and two shell butt straps taken from a steel steamer, are specially interesting, both as a testimony to Siemens-Martin steel—of which that vessel was constructed—and for the care with which they were conducted under the most careful supervision, including representatives of Lloyd's Register. I was personally present at the investigation, and am permitted to embody the results in this Paper.

The objects with which these tests were made was to find out what light could be thrown upon the following propositions, and to what extent they could be verified or disproved by actual experiment:—

1. That great and special attention must be paid to steel ships, because the material, steel, is of such a peculiar and novel character.

2. That damaged steel-plates could not be taken off and re-rolled to go back again in their places, even steel plates in a ship 12 months old, on the ground that they would be so strained that it would not be known or seen how far they had been weakened by the straining.

3. That in a steel steamer only 12 months old, the material, when it came off, must be regarded as second-class material, and not fit to repair the ship with; or, if put back, the vessel would not be eligible to take the 100A class again.

4. That by cutting out the rivets in steel plates the character of steel is very materially injured. By cutting out the rivets and cutting off the heads of the rivets with a cold chisel, a great and injurious strain is brought on the plates in the immediate vicinity of the rivet holes, altering the character of the material very much.

From the detailed analysis of the tests made, Table VII., there is no evidence of loss of strength or of ductility in the material in proximity to the rivet holes of landings or butts more than is to be attributed to the processes of punching, riveting, &c., and no evidence of unseen injury by straining of the ship, or by fairing or rolling, either hot or cold.

The means of 37 pieces taken from solid part of plates were 28·5 tons and 24·2 per cent. extension, and of 34 pieces taken between holes of landings and butts, 28·8 tons and 20 per cent. extension.

In the construction of steel vessels built to Lloyd's Rules, and receiving their highest class under special survey, the option has been, and still is allowed of riveting

with either iron or steel rivets. As Mr. Wildish in his able Paper of last session puts it :—"Iron rivets in steel plates is not a desirable combination." There are sound reasons against the use of iron rivets in steel steamers (especially in the shell and decks) and only one argument in their favour, that of being cheaper. Our uniform practice has been steel rivets for steel steamers, and with ordinary treble riveted butts and 20 per cent. heavier straps than the plates they connected, we have not (save in one of our earliest steel steamers, built in 1879) had a single complaint as to weakness in the butts of any of them.

This I can confirm from personal examination of many of them in Dry Dock after repeated voyages, and also from appended letters from the superintendents of some of the principal Companies, which have built in this material.* Their experience is valuable as evidence on the important points of strength, durability, corrosion, and safety.

In the face of these facts it is difficult to know where the reason has arisen for the extra riveting in shell and stringer butts embodied in Lloyd's Rules of this year—one of the results being a decided weakening of the butts and a partial neutralising of the advantage of a thicker butt strap. If steel vessels have been showing symptoms of weakness at the butts, have they been riveted with iron or steel rivets and are they many in number ?

Steel vessels under the rules of the late Liverpool Registry (now absorbed by Lloyd's) were compelled to be riveted with steel rivets, and a similar practice is carried out in all recent Admiralty work. This matter will doubtless receive early recognition at the hands of Lloyd's Register, and thus give steel vessels all the benefit of constructive thoroughness they are entitled to ; for, apart from the difference in the shearing strength of iron as against steel rivets, another and much more serious defect can be proved to result from the use of iron rivets either in steel or iron plates.

The accompanying series of tests of iron and steel rivets, Table VIII, were most carefully carried out by us in our works, and showed that with no straining, save that due to actual riveting, caulking, and contraction in cooling, a large proportion of the iron rivets on cutting out (after a fortnight's immersion in our dock) had fractures such as shown on diagrams (Plate X.), localised chiefly at the juncture of countersink and shank, where they broke ; the shank and head in the majority of cases coming out together, while the steel

* The letters referred to were from the Marine Superintendent of the Peninsular and Oriental Steamship Company, the Marine Superintendent of the British India and Association Line of steamers, the General Superintendent of the Allan Line of steamers and clippers, and the General Superintendent of the Union Steamship Company of New Zealand ; and they strictly bear out the above statement.—[Ed.]

rivets in every instance were absolutely free from fracture of any kind, and took from three to four times the number of blows to cut out that the iron ones did; and in almost every case the cutting out was only effected when the neck was cut completely through.

The riveting was done by journeymen and differed in no way from work done in actual vessels, and the iron rivets were from those in use by three firms (besides ourselves) who are building vessels to highest class at Lloyd's.

The cause of this investigation was the finding of a large number of apparently sound rivets (in so far as the ordinary method of testing could discover) showing blackened fractures when cut out of a damaged steel steamer riveted with iron rivets.

In cutting out a number of rivets apparently equally sound, in an undamaged iron steamer, a similar result was found, the fractures in these and in our own tests being, as in the steel steamer, principally at the juncture of countersink with shank.

The tests made from the plates in which the rivetings were done, *see* Plate XV., Table IX., go to prove, as do also the tests made last year, recorded in Tables V., VI. and VII., that there is little difference in the nature of the material after punching and repeated rivetings, caulkings and cutting out of rivets as against punching only and without work upon it, and that even tests from the solid show but little superiority over either.

It has been asserted that severe and injurious strains are brought on the plates in the immediate vicinity of the rivet holes through riveting and cutting out. These tests show that either the annealing effect of a hot rivet counteracts this or that the injury is purely imaginary.

Local annealing or rimering such as laid down by Lloyd's for garboards, sheer strakes and stringers seems an undesirable nursing of steel at this period of its age, for if it be necessary to so specially treat the parts mentioned, it should be just as necessary in the bottoms, sides and bilges.

This, experience does not warrant, and probably the better plan would be to remove the special treatment laid down for those portions of the hull, and put as much proportionate faith in the goodness of Siemens-Martin steel as has, without question, been given to iron until now.

The effects of galvanising steel plates and angles, for special work, and where cementing is inadmissible, is a subject with which we have experimented most carefully,

and believe the accompanying tabulated results (Tables X., XI., XII. and XIII.) may prove helpful to members engaged in work of a similar character.

In vessels designed for work in shallow rivers (of which we construct a good many) the lightest possible scantling and preservation of same from corrosion, are two of the points with which we have had to deal.

We early adopted mild steel as the proper material for this type of construction, but the necessity for preservation of same from corrosion in foreign rivers was forced upon us at a later date.

Preservation from corrosion, without deteriorating the material, was the object we had in view in carrying out our first series of galvanising experiments on steel and iron plates.

In the case of the former the results were most satisfactory, the advantages gained being the desired preservation together with a slight increase of tensile strength without any material reduction in its ductility.

In the case of the iron plates galvanising had a weakening effect.

As the result of our experiments galvanising is now adopted by us throughout in all our special light draught work. In our most recent cellular-bottomed steamers of large tonnage we have, to prevent the rapid corrosion which often takes place through the action of heat and bilge water, galvanised the plates of inner bottom which came under main boilers, and in a number of large paddle steamers we have galvanised everything under the boilers for the same reason.

The experiments shewing loss by pickling, also percentage of increase of weight, &c., through galvanising, were most carefully carried out by us at the Steel Company of Scotland's Blochairn and Hallside Works, through the kind permission of Mr. Riley; while we are similarly indebted to Messrs. Beardmore for their kind permission to carry out at their Parkhead Works in 1884 the tests which decided us upon the practice of galvanising now adopted. *See* Tables X., XI., XII. and XIII.

In conclusion, the present aspect of this material, as based on experience, is one of great encouragement both to users and makers, inasmuch as people see reason to forget, or have already forgotten, many of the fears they had at first regarding it.

Our experience in 80 steel vessels launched or delivered with no special treatment or precaution different from that given to iron (save the annealing of butt straps of $\frac{8}{16}$" and upwards, as required in vessels classed at Lloyds'), warrants us in thus making public testimony in its favour.

Basic and Bessemer mild steel for shipbuilding purposes we cannot say much about, as our experience of both has been small, but doubtless some of the members will, in the discussion on this Paper, give us the benefit of their experience and the reasons for the faith they hold regarding all three processes.

The whole subject is one of great and general interest to us all, and if the record of work done in Leven Shipyard in this material since its adoption in 1878, is at all helpful either in adding to the reliable data contained in the Transactions of this Institution or of dispelling some of the fears with which this subject is often invested, then this Paper will have fulfilled the purpose for which it was written.

TABLE I.

EXPERIMENTS ON LOCAL HEATING MADE 17TH AND 18TH MARCH, 1886, IN LEVEN SHIPYARD.

1. PLATE $\frac{5}{8}$ IN. CLYDESDALE STEEL.—A row of 1 in. holes punched down centre of plate, same spacing as for ordinary frames (*see* Fig. 1, Plate X.)

A ring of clay 18 in. diameter, built round part in centre to be heated, water kept running over part outside of ring. The plate was heated on a smith's hearth, the hot part being under the 18 in. circle, the rest being composed of damp green coal. Plate on fire $\frac{1}{4}$ hour. When taken off, it was at a good working red heat and dished upwards owing to the expansion of the heated part. Plate was then laid on blocks and hammered continuously for eight minutes on line between hot and cold by four men using 14 lb. hammers.

When this beating was over, dishing was measured and found to be $1\frac{1}{4}$ in. on a diameter of 16 in. When nearly cool, and when black, plate was beaten in same way on both sides, same was repeated when cold.

When cold it was found that plate had stretched $\frac{1}{16}$ in. in length, and nothing appreciable in breadth.

2. PLATE $\frac{5}{8}$ IN. CLYDESDALE STEEL.—Double row of holes punched along one edge as in ordinary landing (*see* Fig. 2, Plate X.)

A half ring of clay was built on top of plate close to punched edge, ring about 2 ft. diameter and $2\frac{1}{2}$ ins. thick; a second half ring was put on about 3 ins. outside, the space between being kept full of water. The rest of the plate was also kept covered with water. The plate was heated on a smith's hearth, as in last experiment, for 25 minutes.

The plate was then placed on an anvil, and struck on projecting parts round line between hot and cold for about ten minutes by two men using 14 lb. hammers. The plate was then found to be dished $1\frac{3}{4}$ in. in a length of 16 ins. measured along the length, and 11 ins. measured in from edge; after cooling about ten minutes the plate was struck twenty blows on the reverse side while lying on blocks, and after ten minutes more, twenty blows on top side round ring.

The plate was at this time quite black, though the first twenty blows were struck when at a blue heat. About four hours later when quite cold, the plate was beaten in same fashion on top of dishing, when lying on floor of smithy.

3. Plate of $\frac{5}{8}$ in. Clydesdale Steel 5 ft. $10\frac{1}{2}$ ins. by 37 ins. by 26 ins.—Plate bent cold (*see* Fig. 3, Plate X.), then a small circle in centre heated to a dull red, and dished $1\frac{1}{4}$ in. deep by three men using 14 lb. hammers.

Plate heated in same place to a blue heat and bent to the reverse curve in the rolls, the rolling flattening the dent to $\frac{7}{8}$ in.

Plate heated in same place to a blue heat and taken to the rolls and straightened out. It was then taken to the steam-hammer and had the dent flattened out so that the plate was restored to its original condition, the plate was then measured and found of same dimensions as before.

TABLE II.

Experiments on "Dishing," &c., made at Hallside, March, 1886.

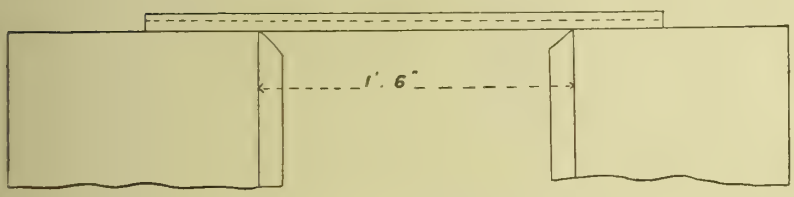
Four plates, $\frac{1}{4}$ in., $\frac{3}{8}$ in., $\frac{1}{2}$ in., $\frac{5}{8}$ in., were taken of ordinary shipyard steel and treated in the following manner:—Tensile tests along and across were cut and treated as in Table IV. Also, three pieces, marked A, B, C, each 2 ft. 6 ins. square, were subjected to bulging by an explosion of dynamite and were afterwards straightened by different methods. A, straightened cold, under steam hammer; B, straightened at bright red heat by being beaten with wooden hammers and then put through rolls; C, straightened at blue heat under steam hammer. The blue heat was determined by the temperature at which tallow just ceased to flash.

After the plates were straightened, tensile tests were cut from the damaged parts, as shown on the accompanying sketches.

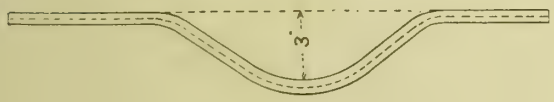
Plate A. No. 1. $\frac{1}{4}$ inch.

Straightened Cold; Tensiles Cut Lengthways.

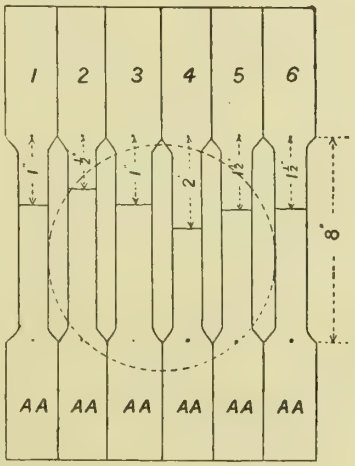
Section before tested.



Section through centre after testing.



Dynamite test.



Damaged part 8 ins. diam., as shown by dotted line.

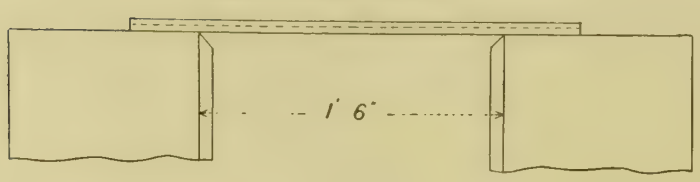
No. of tests.	Tons per square inch.	Extension on 8 inches.	Original thickness.	Thickness of tests.	Nature of fracture.	Distance from punch mark to break.
AA 1	30·7	14·0 per cent.	·28	·275	Irregular.	1 in. from punch mark.
„ 2	31·4	8·0 „	·275	·270	„	½ „ „ „
„ 3	32·3	9·5 „	·27	·26	„	1 „ „ „
„ 4	32·5	7·5 „	·275	·26	„	2 „ „ „
„ 5	31·2	10·0 „	·28	·27	Cup.	1½ „ „ „
„ 6	30·3	10·5 „	·29	·28	„	1½ „ „ „

¼ Inch Ship Plate—*continued*.

Plate B., No. 1 (Cut from same Plate as A 1.)

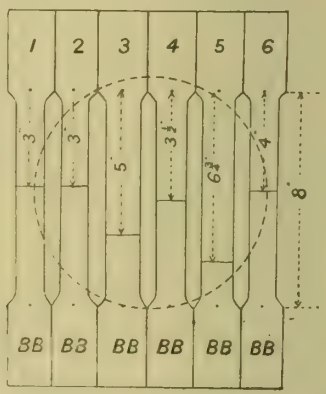
Straightened Hot, Tensiles Cut Lengthways.

Section before tested.



Section through centre after testing.

Dynamite test.



Damaged part 11 ins. diam., as shown by dotted line.

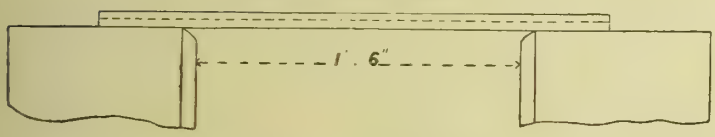
No. of tests.	Tons per square inch.	Extension on 8 inches.	Original Thickness.	Thickness of test.	Nature of fracture.	Distance from punch mark to break.
B. B. 1	28·6	23·0 per cent.	·28	·275	Diagonal.	3 ins. from punch mark.
,, 2	29·4	23·5 ,,	·26	·25	do.	3 ,, ,, ,,
,, 3	28·9	17·0 ,,	·26	·24	Cup.	5 ,, ,, ,,
,, 4	28·4	16·0 ,,	·25	·26	Irregular.	3½ ,, ,, ,,
,, 5	27·9	20·5 ,,	·27	·265	do.	6¾ ,, ,, ,,
,, 6	28·3	25·0 ,,	·27	·270	Cup.	4 ,, ,, ,,

¼ Inch Ship Plate—*continued.*

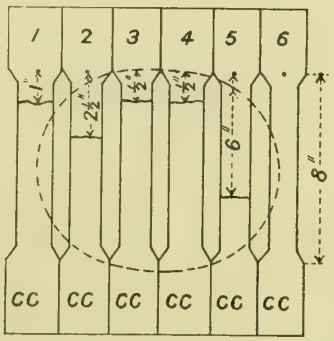
Plate C., No 1 (Cut from same Plate as A. and B. No. 1).

Straightened at a Blue Heat, Tensiles Cut Crossways.

Section before tested.



Section through centre after testing.



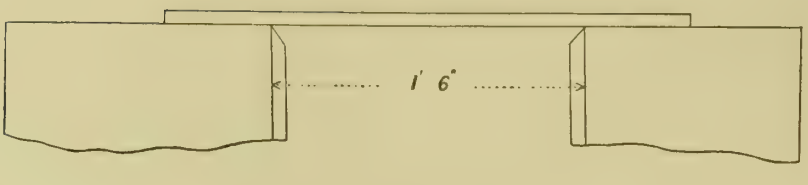
Damaged part 10 ins. × 8 ins. diam., as shown by dotted line.

No. of tests.	Tons per square inch.	Extension on 8 inches.	Original thickness.	Thickness of test.	Nature of fracture.	Distance from punch mark to break.
C. C. 1	33·3	5·0 per cent.	·29	·28	Irregular.	1 in. from punch mark.
„ 2	35·1	6·0 „	·28	·275	do.	2½ „ „ „
„ 3	33·9	2·5 „	·29	·28	do.	½ „ „ „
„ 4	33·9	3·0 „	·285	·28	do.	½ „ „ „
„ 5	33·6	5·0 „	·285	·275	do.	6 „ „ „
„ 6	23·5	4.0 „	·280	·27	do.	Broke through punch mark.

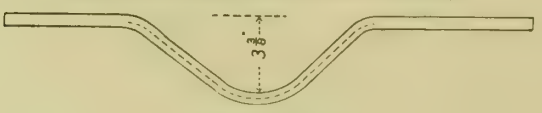
PLATE A. No. 2. $\frac{3}{8}$ IN.

STRAIGHTENED COLD, TENSILES CUT LENGTHWAYS.

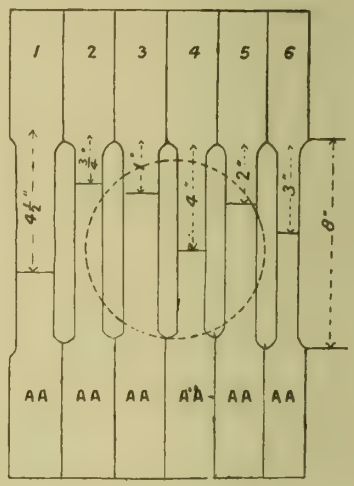
Section before tested.



Section through centre after testing.



Dynamite test.



Damaged part 7 in. diam., as shown by dotted line.

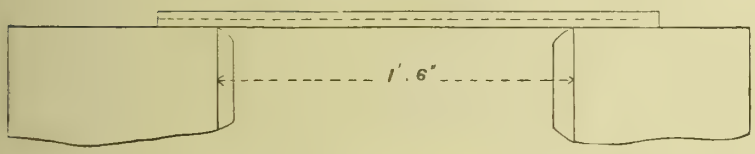
No. of test.	Tons. per square inch.	Extension on 8 inches.	Original thickness.	Thickness of test.	Nature of fracture.	Distance from punch mark to break.
AA 1	29·9	19·0 per cent.	·38	·37	Cup.	4½ ins. from punch mark.
,, 2	29·9	8·5 ,,	·38	·37	Diagonal.	¾ ,, ,,
,, 3	29·9	7·0 ,,	·38	·37	Cup.	1 ,, ,,
,, 4	30·8	4·0 ,,	·375	·36	Diagonal.	4 ,, ,,
,, 5	30·5	10·0 ,,	·375	·36	Irregular.	2 ,, ,,
,, 6	30·5	17·0 ,,	·38	·37	Diagonal.	3 ,, ,,

$\frac{3}{8}$ Ship Plates—*continued.*

Plate B. No. 2 (Cut from same Plate as A. No. 2.)

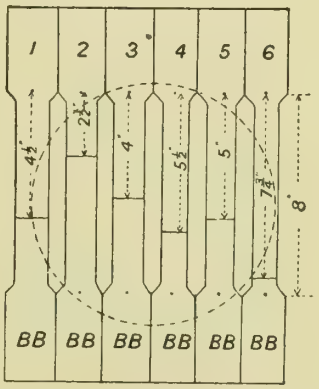
Straightened Hot, Tensiles cut Crossways.

Section before tested.



Section through centre after testing.

Dynamite test.



Damaged part shown by dotted line 12 ins. diam.

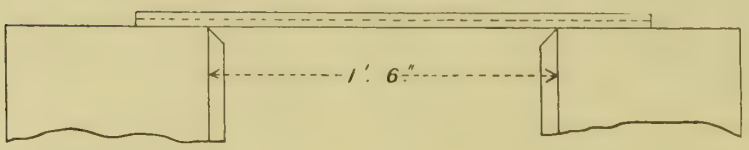
No. of test.	Tons per square inch.	Extension on 8 inches.	Original thickness.	Thickness of test.	Nature of fracture.	Distance from punch mark to break.
BB 1	28.8	20·0 per cent.	·365	·35	Irregular.	4½ ins. from punch mark.
,, 2	28.4	21·0 ,,	.370	·36	Diagonal.	2½ ,, ,, ,,
,, 3	28·2	21·5 ,,	·370	·36	Irregular.	4 ,, ,, ,,
,, 4	28·0	18·0 ,,	·375	·36	Diagonal.	5½ ,, ,, ,,
,, 5	28·2	21·5 ,,	·370	·36	Irregular.	5 ,, ,, ,,
,, 6	28·0	23·5 ,,	·370	·36	Diagonal.	7¾ ,, ,, ,,

$\frac{3}{8}$ Ship Plate—*continued.*

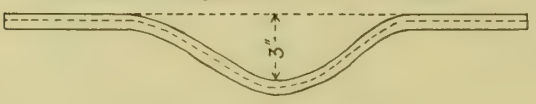
Plate C. No. 2 (Cut from same Plate as A. and B. No. 2).

Straightened at Blue Heat, Tensiles cut Crossways.

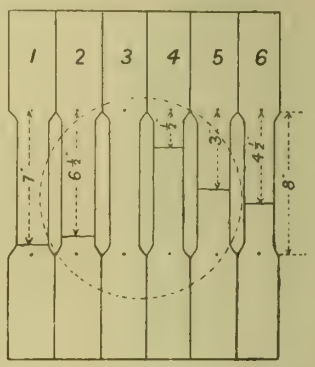
Section before tested.



Section through centre after testing.



Dynamite test.



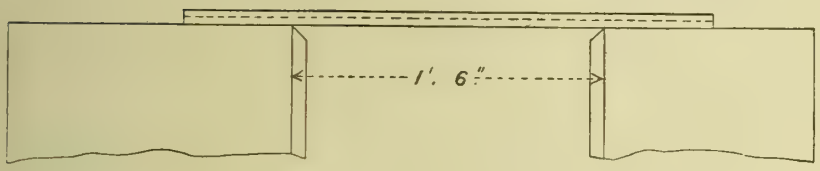
Damaged part shown by dotted line 9 ins. diam.

No. of test.	Tons per square inch.	Extension on 8 inches.	Original thickness.	Thickness of piece.	Nature of fracture.	Distance from punch mark to break.
C C 1	34·9	10·5 per cent.	·38	·37	Irregular.	7 ins. from punch mark.
,, 2	32·6	5·6 ,,	·38	·37	,,	6½ ,, ,, ,,
,, 3	32·6	2·5 ,,	·38	·37	,,	Broke at punch mark.
,, 4	31·9	5·0 ,,	·38	·37	,,	½ in. from punch mark.
,, 5	33·1	7·0 ,,	·38	·37	Cup.	3 ,, ,, ,,
,, 6	33·7	8·0 ,,	·38	·37	Irregular.	4½ ,, ,, ,,

Plate A, No. 3. ½ Inch.

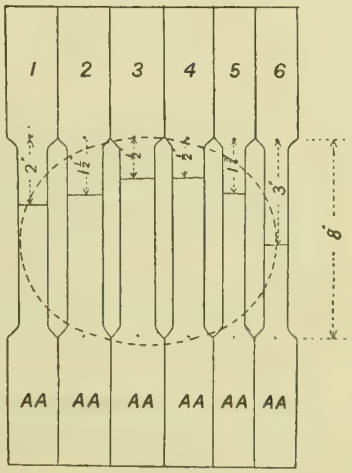
Straightened Cold, Tensiles Cut Crossways.

Section before tested.



Section through centre after testing.

Dynamite test.



Damaged part 12 ins. × 10 ins., as shown by dotted line.

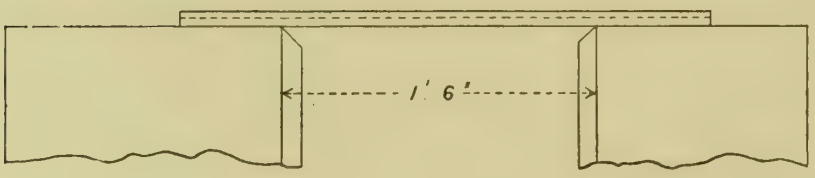
No. of test.	Tons per square inch.	Extension on 8 inches.	Original thickness.	Thickness of test.	Nature of fracture.	Distance from punch mark to end.
A A 1	27·8	20·5 per cent.	·50	·49	Irregular.	2 ins. from top punch mark.
,, 2	28·2	11·5 ,,	·50	·49	Diagonal.	1½ ,, ,, ,,
,, 3	28·3	10·0 ,,	·50	·49	,,	½ ,, ,, ,,
,, 4	27·9	18·0 ,,	·505	·50	Irregular.	½ ,, ,, ,,
,, 5	27·8	18·0 ,,	·50	·49	Diagonal.	1¾ ,, ,, ,,
,, 6	27·8	18·5 ,,	·50	·49	,,	3 ,, ,, ,,

½ Inch Ship Plate—*continued.*

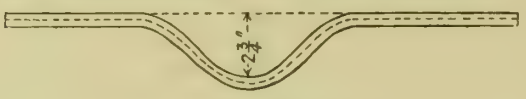
Plate B, No. 3 (cut from same Plate as A, No. 3.)

Straightened Hot, Tensiles Cut Lengthways.

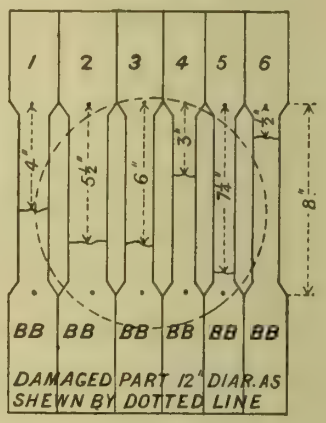
Section before tested.



Section through centre after testing.



Dynamite test.



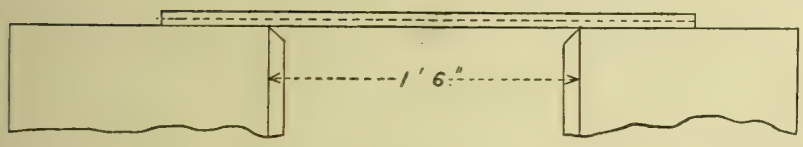
No. of test.	Tons per square inch.	Extension on 8 inches.	Original Thickness.	Thickness of piece.	Nature of fracture.	Distance from punch mark to break.
B B 1	28·4	22·5 per cent.	·50	·495	Cup.	4 ins. from top punch mark.
,, 2	28·3	22·5 ,,	·50	·49	,,	5½ ,, ,, ,,
,, 3	27·9	18·5 ,,	·50	·49	Diagonal.	6 ,, ,, ,,
,, 4	28·6	18·5 ,,	·50	·50	,,	3 ,, ,, ,,
,, 5	28·2	21·0 ,,	·515	·51	Cup.	7¼ ,, ,, ,,
,, 6	27·9	22·5 ,,	·515	·51	Diagonal.	½ ,, ,, ,,

½ Inch Ship Plate—*continued.*

Plate C, No. 3 (cut from same Plate as A and B 3.)

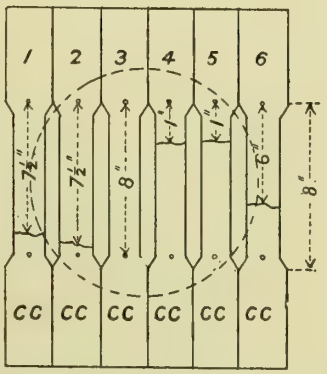
Straightened at a Blue Heat, Tensiles Cut Lengthways.

Section before tested.



Section through centre after testing.

Dynamite test.



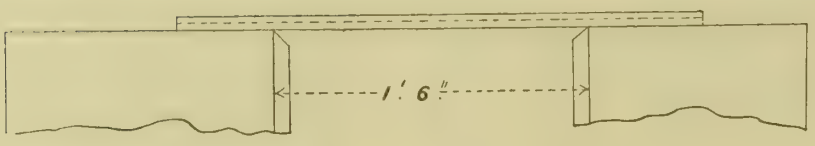
Damaged part 10 in. by 9 in., as shown by dotted line.

No. of Test.	Tons per square inch.	Extension on 8 inches.	Original Thickness.	Thickness of test.	Nature of Fracture.	Distance from punch mark to break.
C C 1	31·9	10·5 per cent.	·49	·48	Cup.	7½ ins. from top punch mark.
,, 2	32·4	7·5 ,,	·49	·48	Diagonal.	7½ ,, ,, ,,
,, 3	31·2	6·5 ,,	·50	·49	,,	8 ,, ,, ,,
,, 4	32·4	4·0 ,,	·495	·48	,,	1 ,, ,, ,,
,, 5	31·2	7·5 ,,	·495	·485	Irregular.	1 ,, ,, ,,
,, 6	31·2	12·0 ,,	·495	·485	Diagonal.	6 ,, ,, ,,

Plate A. No. 4. $\frac{5}{8}$ Inch.

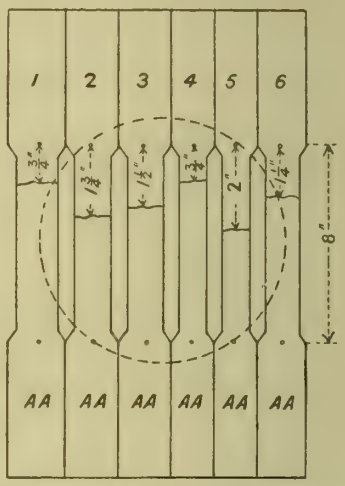
Straightened Cold, Tensiles Cut Crossways.

Section before tested.



Section through centre after testing.

Dynamite test.



Damaged part 10 in. × 11 in. as shown by dotted line.

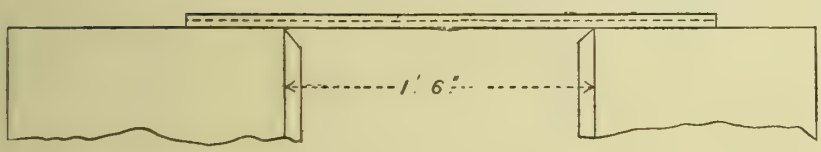
No. of test.	Tons per square inch.	Extension on 8 inches.	Original Thickness.	Thickness of test.	Nature of fracture.	Distance from punch mark to break.
A A 1	27·9	17·5 per cent.	...	·64	Irregular.	$\frac{3}{4}$ inch from top punch mark.
,, 2	27·5	14·0 ,,	...	·64	,,	$1\frac{3}{4}$,, ,, ,,
,, 3	28·3	12·0 ,,	...	·64	,,	$1\frac{1}{2}$,, ,, ,,
,, 4	28·5	11·0 ,,	...	·63	,,	$\frac{3}{4}$,, ,, ,,
,, 5	27·8	17·0 ,,	...	·64	,,	2 ,, ,, ,,
,, 6	27·9	20·0 ,,	...	·64	Diagonal.	$1\frac{1}{4}$,, ,, ,,

$\frac{5}{8}$ Inch Ship Plate.—*continued.*

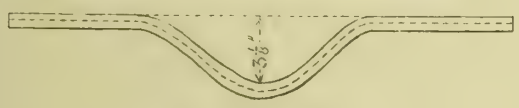
Plate B, No. 4 (cut from same Plate as A No. 4.)

Straightened Hot, Tensiles Cut Crossways.

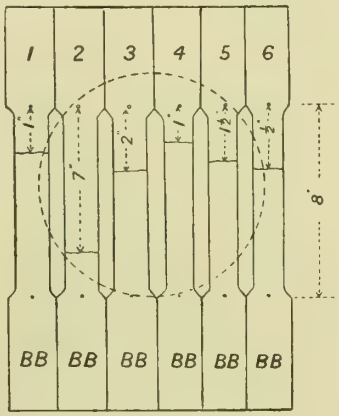
Section before tested.



Section through centre after testing.



Dynamite test.



Damaged part 12 inches diameter, as shown by dotted line.

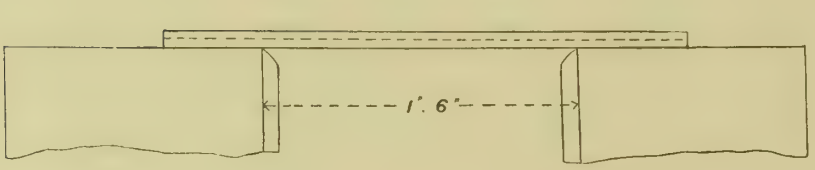
No. of test.	Tons per square inch.	Extension on 8 inches.	Original Thickness.	Thickness of piece.	Nature of fracture.	Distance from punch mark to break.
B B 1	27·8	21·0 per cent.	·64	·63	Diagonal	1 inch from top punch mark.
„ 2	28·4	19·0 „	·64	·63	Irregular	7 „ „ „
„ 3	27·9	17·5 „	·64	·635	„	2 „ „ „
„ 4	28·1	18·0 „	·63	·62	„	1 „ „ „
„ 5	27·8	24·5 „	·64	·63	„	1½ „ „ „
„ 6	28·1	24·5 „	·63	·62	Diagonal	2 „ „ „

$\frac{3}{8}$ Inch Ship Plate—*continued.*

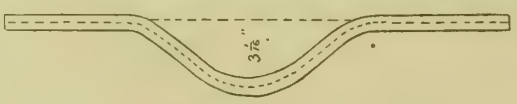
Plate C, No. 4 (cut from same Plate as A and B 4.)

Straightened at a Blue Heat. Tensiles cut Lengthways.

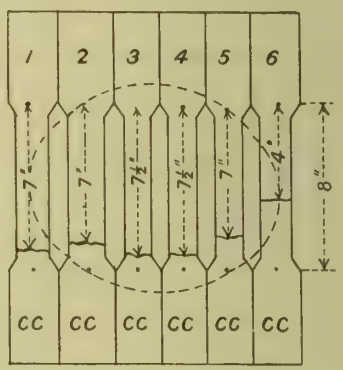
Section before tested.



Section through centre after testing.



Dynamite test.



Damaged part 13 ins. × 10 ins. as shown by dotted line.

No. of test.	Tons per square inch.	Extension on 8 inches.	Original thickness.	Thickness of piece.	Nature of Fracture.	Distance from punch mark to break.
C C 1	31·5	11·0 per cent.	·64	·635	Irregular	7 inches from top punch mark.
,, 2	32·0	10·0 ,,	·64	·630	Diagonal	7 ,, ,, ,,
,, 3	33·3	6·5 ,,	·64	·630	Cup	7½ ,, ,, ,,
,, 4	33·9	8·0 ,,	·63	·620	,,	7½ ,, ,, ,,
,, 5	32·9	7·0 ,,	·64	·635	Irregular	7 ,, ,, ,,
,, 6	31·7	13·5 ,,	·63	·625	,,	4 ,, ,, ,,

PRESENT ASPECT OF MILD STEEL FOR SHIPBUILDING.

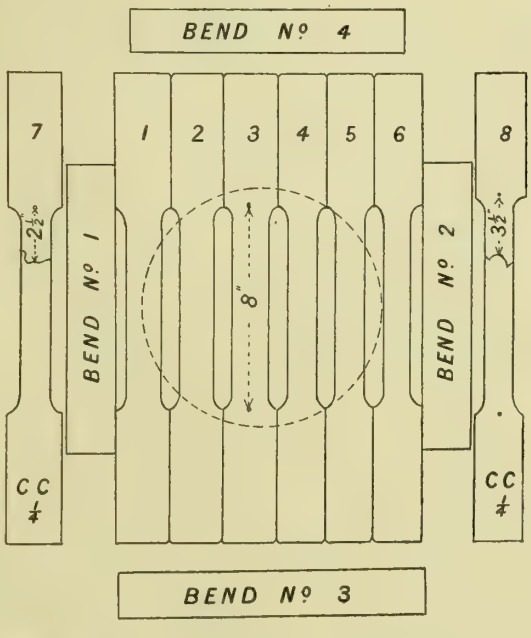
Experimental Tests of $\frac{1}{4}$ in. and $\frac{3}{8}$ in. Ship Plates.

Two Additional Tensiles and Four Bends cut from each of the Plates which were Straightened at a Blue Heat, but Outside of Part Bulged by Dynamite.

No. of test.	Tons per square inch.	Extension on 8 inches.	Remarks.	Nature of fracture.
C C 7	30·9	18·0%	Cut crossways.	Irregular
C C 8	30·9	17·0%	do.	Cup.

Bends.

Bend No.	Long or cross.	Remarks.	
No. 1	Crossways	Bent to $\frac{1}{2}$ in. rad.	Slight crack on edge.
,, 2	do.	Do.	do.
,, 3	Lengthways	Do.	Good.
,, 4	do.	Bent to $\frac{3}{8}$ in. radius.	Good.

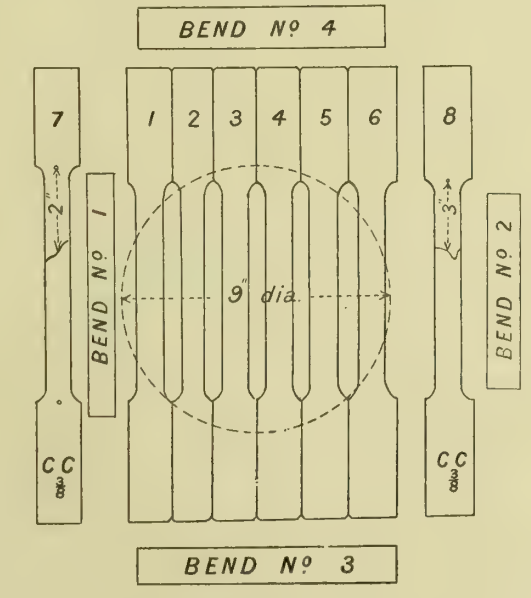


$\frac{1}{4}$ in. Plate Tensiles cut Crossways.

No. of test.	Tons per square inch.	Extension on 8 inches.	Nature of fracture.	Long or cross.
C C 7	31·2	16·0%	Diagonal	Crossways
C C 8	31·3	17·0%	Irregular	do.

Bends.

Bend No.	Long or Cross.	Remarks.	
No. 1	Crossways	Bent $\frac{9}{16}$ in. rad.	Slight crack on edge.
,, 2	do.	Do.	Good.
,, 3	Lengthways	Do.	Good.
,, 4	do.	Bent to $\frac{1}{2}$ in. rad.	Good.



$\frac{3}{8}$ in. Plate Tensiles cut Crossways.

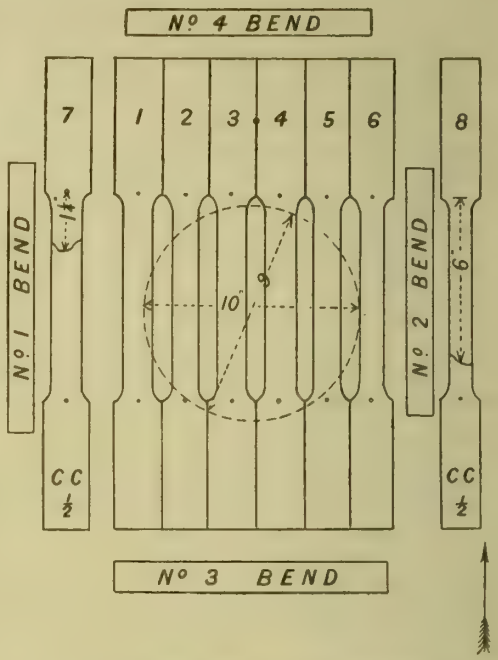
Experimental Tests of ½ in. and ⅝ in. Ship Plates.

Two additional Tensiles and four Bends cut from each of the Plates which were Straightened at a Blue Heat, but outside of part Bulged by Dynamite.

No. of Test.	Tons per square inch.	Extension on 8 inches.	Nature of fracture.	Long or cross.
C C 7	29·1	21·0	Irregular	Lengthways.
C C 8	28·8	22·5	Diagonal	,,

Bends

Bend No.	Long or cross.	Remarks.
No. 1 .	Long	Bent to radius of ¾ in. Good.
,, 2 .	,,	,, ,, ,, ,,
,, 3 .	Cross	,, ,, ,, ,,
,, 4 .	,,	,, ,, ,, ,,

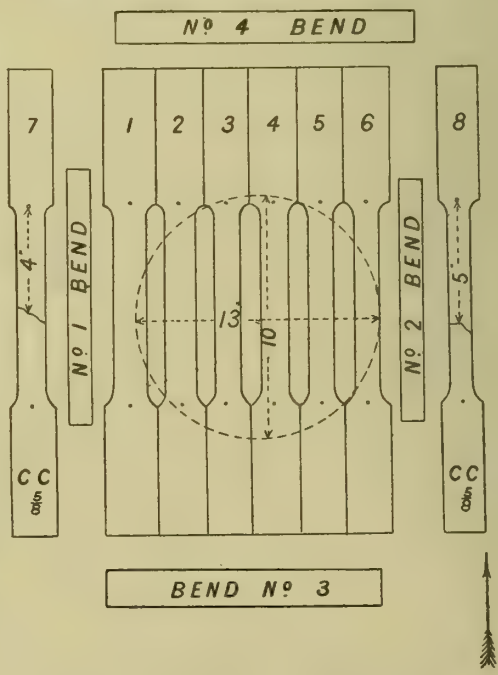


½ in. Plate Tensiles cut Lengthways.

Test No.	Tons per square inch.	Extension on 8 inches.	Nature of fracture.	Long or cross.
C C 7	28·8	22·0	Diagonal	Lengthways.
C C 8	29·0	19·5	Irregular	,,

Bends.

Bend No.	Long or cross.	Remarks.
No. 1 .	Long	Bent to a radius of ⅞ in. Good.
,, 2 .	,,	,, ,, ,, ,,
,, 3 .	Cross	,, ,, ,, ,,
,, 4 .	,,	,, ,, ,, ,, Cracked on one edge.



⅝ in. Plate Tensiles cut Lengthways.

TABLE III.

Comparison of strength and ductility of the Steel Plates dished as shown in Table II. and straightened at blue heat, with ordinary Shipyard Iron.

STEEL PLATES DISHED WITH DYNAMITE AND STRAIGHTENED AT A BLUE HEAT.

¼ in. plate	No. of Test	1 C.	2 C.	3 C.	4 C.	5 C.	6 C.
	Tons per sq. in	33·3	35·1	33·9	33·9	33.6	33·3
	Extension on 8 in. per cent.	5·0	6·0	2 5	3·0	5·0	4·0
⅜ in. plate	No. of Test	1 C.	2 C.	3 C.	4 C.	5 C.	6 C.
	Tons per sq. in.	31·9	32·6	32·6	31·9	33·1	33·7
	Extension on 8 in. per cent.	10·5	5·6	2·5	5 0	7·0	8 0
½ in. plate	No. of Test	1 C.	2 C.	3 C.	4 C.	5 C.	6 C.
	Tons per sq. in.	31·9	32·4	31·2	32 4	31·2	31 2
	Extension on 8 in. per cent	10·5	7·5	6·5	4·0	7·5	12·0
⅝ in. plate	No. of Test	1 C.	2 C.	3 C.	4 C.	5 C.	6 C.
	Tons per sq. in.	31 5	32·0	33·3	33·9	32·9	31·7
	Extension on 8 in. per cent	11·0	10·0	6·5	8·0	7 0	13·5

Results of Tests on Iron Plates.

Taken from "Fairbairn's Iron Shipbuilding," 1865.

A. 2 specimens of Iron Plate
B. 3 ,, ,,
C. 3 ,, homogeneous metal
D. 3 ,, rolled iron
} by four different firms

Thickness.	Breaking stress per square inch in tons.				Elongation per cent.			
	A.	B.	C.	D.	A.	B.	C.	D
¼ inch . . .	24·344	24·167	30·703	17·470	6·2	3·0	25·6	·8
½ inch . . .	25·750	23·220	33 694	11·055	7·6	4·0	10·0	1·0
¾ inch . . .	...	29 432	30·913	26·473	...	10·0	20·8	4·0
Mean . . .	25·047	25 606	31·770	18·333	6·9	5·7	18·8	2·0
Mean of A., B. and D., C. being apparently similar material to steel.			22·995.			4·86 per cent.		

Results of Experiments made at Leven Shipyard, July, 1879, on Six Specimens of Consett Iron cut from Two Plates.

Mark of Test.	½ inch.			⅜ inch.		
	1	4	6	2	3	5
Tons per square inch . .	20·6	22·7	21·1	22·4	21·9	21·6
Extension per cent. on 8 ins.	1·6	2·0	1·5	2·7	4·5	4·0

Results of Experiments on Iron Ship Plates made at Parkhead Iron and Steel Works, 6th November, 1884.

These tests are taken from Table X.

No. of Test.	1	2	3	4	5	6		8
Tons per square inch. .	20·1	22·3	18·5	22·7	19·8	21·7	23·5	24·7
Extension per cent. on 8 ins.	1	4	1	2	5	3	4	5

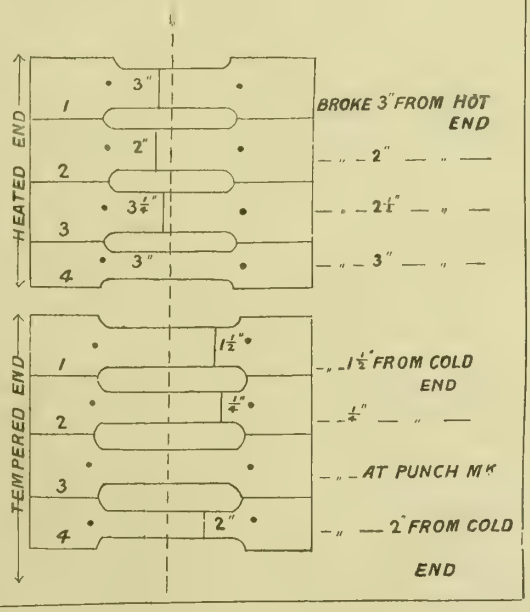
TABLE IV.

Result of testing pieces taken from plates as described in Table II. subjected to different heatings

¼ Inch Ship Plate.

Tensile Tests cut from same Plate as A, B and C 1.

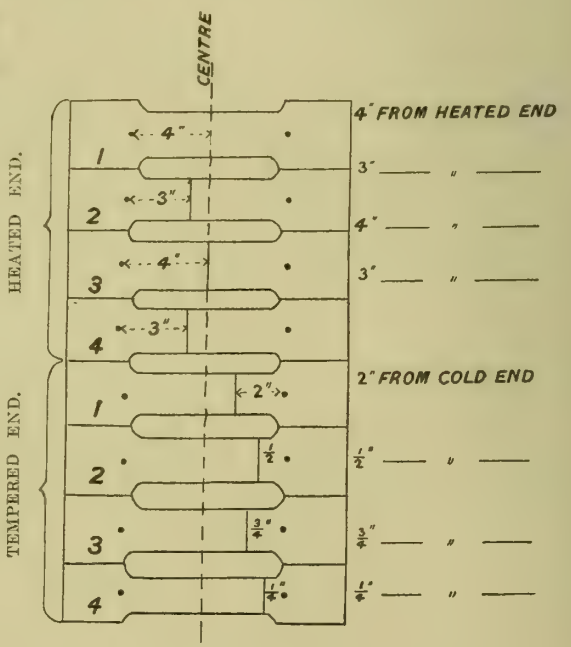
Mark & No.	Tons per square inch.	Extension on 8 inches.	Long or Cross.	Remarks.
A 1	29·2	21·5 per cent.	Long	Tested as cut from plate.
,, 2	29·3	23·0 ,,	,,	
,, 3	29·3	18·5 ,,	Cross	
,, 4	29·5	20·5 ,,	,,	
B 1	26·8	28·0 ,,	Long	Do. annealed.
,, 2	27·2	22·5 ,,	,,	
,, 3	27·3	23·0 ,,	Cross	
,, 4	28·0	21·5 ,,	,,	
C 1	30·2	23·5 ,,	Long	Do. heated cherry red and cooled in water 82° Fahr.
,, 2	30·5	21·5 ,,	,,	
,, 3	30·9	19·5 ,,	Cross	
,, 4	29·8	19·0 ,,	,,	
C 1 ×	34·7	17·0 ,,	Long	Do. heated bright red and cooled in water 82° Fahr.
,, 2 ×	33·3	14·0 ,,	,,	
,, 3 ×	35·3	15·5 ,,	Cross	
,, 4 ×	35·9	15·0 ,,	,,	
D 1	30·9	20·0 ,,	Long	Do. heated to blue heat and cooled in air.
,, 2	30·9	22·5 ,,	,,	
,, 3	30·4	18·0 ,,	Cross	
,, 4	30·0	18·0 ,,	,,	
E 1	30·3	21·5 ,,	Long	Do. heated to blue heat and cooled in water.
,, 2	31·3	18·5 ,,	,,	
,, 3	30·4	16·5 ,,	Cross	
,, 4	30·6	14·0 ,,	,,	
F 1	28·4	21·0 ,,	Long	Heated red halfway and cooled in air
,, 2	28·6	25·0 ,,	,,	
,, 3	28·7	19·5 ,,	Cross	
,, 4	28·8	20·0 ,,	,,	
G 1	31·2	11·0 ,,	Long	Heated red halfway and cooled in water.
,, 2	31·9	10·0 ,,	,,	
,, 3	30·7	6·0 ,,	Cross	
,, 4	31·8	10·0 ,,	,,	



$\frac{3}{8}$ Inch Ship Plate.

Tensile Tests Cut from same Plate as A, B, & C 2.

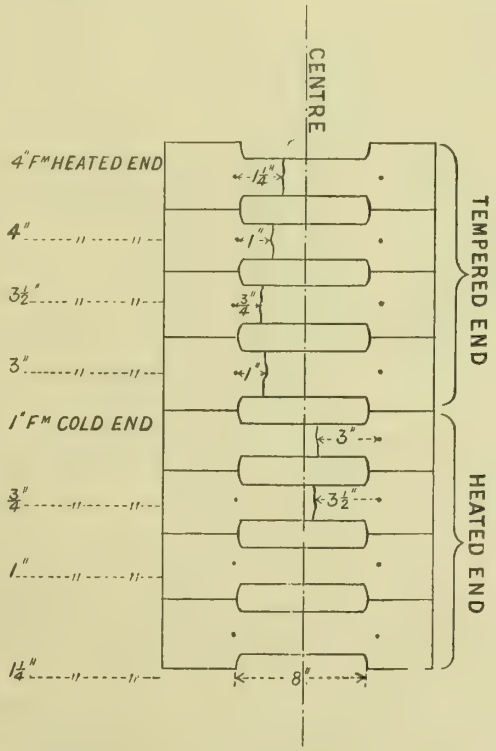
Mark and Number.	Tons per square inch.	Extension on 8 inches.	Long or Cross.	Remarks.
A 1	29·1	26·5 per cent.	Long	Tested as cut from plate.
,, 2	29·1	23·5 ,,	,,	
,, 3	28·9	23·5 ,,	Cross	
,, 4	29·0	21·0 ,,	,,	
B 1	26·5	29·0 ,,	Long	Do. annealed.
,, 2	26·9	27·0 ,,	,,	
,, 3	26·5	26·0 ,,	Cross	
,, 4	26·8	26·0 ,,	,,	
C 1	30·7	27·5 ,,	Long	Do. heated cherry-red and cooled in water 82° Fahr.
,, 2	30·6	27·0 ,,	,,	
,, 3	31·2	23·5 ,,	Cross	
,, 4	30·8	23·5 ,,	,,	
C 1 ×	38·7	4·0 ,,	Long	Do. heated bright red, and cooled in water 82° Fahr.
,, 2 ×	37·3	15·0 ,,	,,	
,, 3 ×	38·9	8·0 ,,	Cross	
,, 4 ×	42·0	8·0 ,,	,,	
D 1	29·5	22·0 ,,	Long	Do. heated to blue heat and cooled in air.
,, 2	29·3	25·0 ,,	,,	
,, 3	29·4	23·0 ,,	Cross	
,, 4	29·4	23·5 ,,	,,	
E 1	29·8	22·0 ,,	Long	Do. heated to blue heat and cooled in water.
,, 2	29·6	21·5 ,,	,,	
,, 3	29·6	22·0 ,,	Cross	
,, 4	29·4	22·5 ,,	,,	
F 1	29·2	23·0 ,,	Long	Heated red half-way and cooled in air.
,, 2	28·9	22·5 ,,	,,	
,, 3	28·8	22·0 ,,	Cross	
,, 4	28·8	20·0 ,,	,,	
G 1	30·0	13·0 ,,	Long	Heated red half-way and cooled in water,
,, 2	30·3	9·0 ,,	,,	
,, 3	30·2	10·0 ,,	Cross	
,, 4	30·3	10·0 ,,	,,	



½ INCH SHIP PLATE.

TENSILE TESTS CUT FROM SAME PLATE AS A, B AND, C 3.

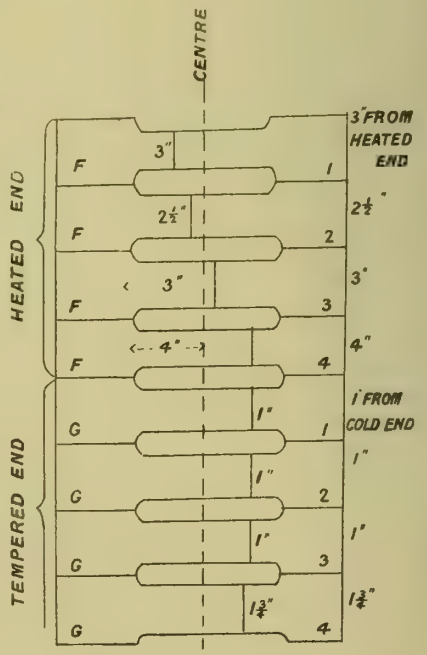
Mark and Number.	Tons per square inch.	Extension on 8 inches.	Long or Cross.	Remarks.
A 1	27·5	25·0 per cent.	Long	Tested as cut from plate.
,, 2	27·5	29·5 ,,	,,	
,, 3	27·8	24·0 ,,	Cross	
,, 4	28·0	24·5 ,,	,,	
B 1	27·2	29·0 ,,	Long	Do. annealed.
,, 2	26·8	25·5 ,,	,,	
,, 3	26·8	20·5 ,,	Cross	
,, 4	27·2	20·5 ,,	,,	
C 1	30·5	19·5 ,,	Long	Do. heated bright red and cooled in water 82° Fahr.
,, 2	30·3	18·0 ,,	,,	
,, 3	32·6	14·0 ,,	Cross	
,, 4	32·6	13·0 ,,	,,	
D 1	28·6	25·0 ,,	Long	Do. heated blue and cooled in air.
,, 2	28·5	25·5 ,,	,,	
,, 3	28·1	25·0 ,,	Cross	
,, 4	28·1	20·0 ,,	,,	
E 1	28·4	24·0 ,,	Long	Do. heated blue and cooled in water.
,, 2	28·4	25·5 ,,	,,	
,, 3	28·0	23·5 ,,	Cross	
,, 4	28·3	25·0 ,,	,,	
F 1	27·4	22·0 ,,	Long	Heated red halfway and cooled in air.
,, 2	27·6	23·0 ,,	,,	
,, 3	27·6	20·0 ,,	Cross	
,, 4	27·6	19·0 ,,	,,	
G 1	27·4	13·0 ,,	Long	Heated red halfway and cooled in water.
,, 2	28·3	12·0 ,,	,,	
,, 3	28·3	13·5 ,,	Cross	
,, 4	28·0	14·5 ,,	,,	



$\frac{5}{8}$ inch Ship Plate.

Tensile Tests from same Plate as A, B, & C 4.

Mark & No.	Tons per square inch.	Extension on 8 inches.	Long or cross.	Remarks.
A 1	28·0	25·0 per cent.	Long	Tested as cut from plate.
,, 2	28·0	23·5 ,,	,,	
,, 3	28·2	26·0 ,,	Cross	
,, 4	27·6	22·5 ,,	,,	
B 1	26·9	25·0 ,,	Long	Do. annealed.
,, 2	27·1	24·5 ,,	,,	
,, 3	27·1	22·0 ,,	Cross	
,, 4	26·8	22·0 ,,	,,	
C 1	31·1	20·5 ,,	Long	Do. heated cherry red and cooled in water 82° Fahr.
,, 2	30·5	23·0 ,,	,,	
,, 3	30·6	20·5 ,,	Cross	
,, 4	30·9	22·5 ,,	,,	
C 1 ×	36·2	11·0 ,,	Long	Do. bright red and cooled in water 82° Fahr.
,, 2 ×	35·6	10·0 ,,	,,	
,, 3 ×	34·6	11·5 ,,	Cross	
,, 4 ×	34·9	8·0 ,,	,,	
D 1	29·2	23·5 ,,	Long	Heated to blue heat and cooled in air.
,, 2	28·8	24·0 ,,	,,	
,, 3	28·4	24·5 ,,	Cross	
,, 4	28·3	22·5 ,,	,,	
E 1	28·1	26·0 ,,	Long	Do. to blue heat and cooled in water.
,, 2	29·1	23·5 ,,	,,	
,, 3	28·6	20·5 ,,	Cross	
,, 4	28·1	23·5 ,,	,,	
F 1	28·4	21·0 ,,	Long	Heated red half-way and cooled in air.
,, 2	28·2	20·0 ,,	,,	
,, 3	27·7	21·0 ,,	Cross	
,, 4	27·8	21·0 ,,	,,	
G 1	29·5	14·0 ,,	Long	Heated red half-way and cooled in water.
,, 2	28·2	14·0 ,,	,,	
,, 3	29·4	11·0 ,,	Cross	
,, 4	28·8	16·0 ,,	,,	



Summary of Preceding Results.

	¼ inch Plate Means.		⅜ inch Plate Means.		½ inch Plate Means.		⅝ inch Plate Means.	
	Tons per square inch.	Extension on 8 inches per cent.	Tons per square inch.	Extension on 8 inches per cent.	Tons per square inch.	Extension on 8 inches per cent.	Tons per square inch.	Extension on 8 inches per cent.
Tested as cut from plate (A)	29·325	20·9	29·025	23·6	27·7	25·8	27·95	24·3
,, Annealed (B)	27·325	23·8	26·675	27·0	27·0	23·9	26·975	23·4
,, Heated to a cherry red and cooled in water at 82° F (C)	30·350	20·9	30·825	25·4			30·775	21·6
,, ,, bright ,, ,, ,, ,,	34·8	15·4	39·225	8·75	31·5	16·1	35·325	10·1
,, ,, blue heat ,, ,, air (D)	30·55	19·6	29·4	23·4	28·325	23·9	28·675	23·6
,, ,, ,, ,, ,, ,, water (E)	30·65	17·6	29·6	22·0	28·275	24·5	28·475	23·4
,, Heated red half way and cooled in air (F)	28·625	21·4	28·925	21·9	27·55	21·0	28·025	20·8
,, ,, ,, ,, ,, ,, water G)	31·400	9·3	30·2	10·5	28·0	13·3	28.95	13·8

TABLE V.

STATEMENT OF TESTS TAKEN FROM PLATES OF DAMAGED STEEL STEAMER (*see* PLATES XI., XII., XIII. AND XIV.).

PLATE E 5.

Aft part of Plate.	No. 1	Test piece between holes of upper landing, rivets drilled out
	,, 2	,, tensile adjoining ,,
	,, 3	,, to be drilled and tested similar to No. 1
	,, 4	,, tensile across the plate
	,, 5	Bend adjoining No. 4 across plate
	,, 6	Punched same as outer holes of butt
	,, 7	Test through outer holes of butt
	,, 8	,, ,, upper holes of bilge keel
	,, 9	,, punched same as ,, ,, ,,
	,, 10	Tensile adjoining upper holes of bilge keel
	,, 11	Bend (with the plate)
	,, 12	,, ,, ,, ,,
Centre of Plate.	,, 13	Bend diagonally from centre part plate
	,, 14	Tensile ,, ,, ,, ,, ,,
	,, 15	,,
	,, 16	Bend
Fore end of Plate.	,, 17	Tensile between rivets in lower landing
	,, 18	,, solid plate adjoining
	,, 19	,, punched same as 17
	,, 20	Bend across plate adjoining lower part of butt
	,, 21	,, ,, ,, ,, ,, ,, ,, ,,
	,, 22	Tensile between rivets, upper landing
	,, 23	,, solid plate adjoining upper landing
	,, 24	,, after punching same as 22
	,, 25	Tensile through outer row of rivet holes, upper part of butt
	,, 26	,, after punching similarly to 25
	,, 27	Tensile across plate

Plate E 6.

Aft end of Plate.

No. 28 Tensile between rivet holes in butt, upper edge
„ 29 „ treated similar to 28 from adjoining solid
„ 30 „ between upper landing holes adjoining butt rivets drilled out
„ 31 „ between holes drilled as in 30
„ 32 Tensile adjoining rivet holes in upper landing
„ 33 Bend with the plate near centre
„ 34 „ „ „ „ „ „
„ 35 Tensile between holes lower landing at butt
„ 36 „ punched and treated same as 35
„ 37 „ through holes of lower bilge fin angle
„ 38 „ punched and treated same as 37
„ 39 „ with the plate and between the holes of bilge fin
„ 40 Bend with the plate near centre
„ 41 „ „ „ „ „ „

Centre of Plate.

„ 42 Tensile across plate
„ 43 Bend „ „
„ 44 Tensile with the plate
„ 45 Bend „ „ „

Fore end of Plate.

„ 46 Tensile between lower landing holes
„ 47 „ punched and treated similar to 46
„ 48 „ from solid above upper holes of bilge keel
„ 49 Bend, with the plate near centre.
„ 50 „ „ „ „ „ „
„ 51 Tensile adjoining lower landing, and at butt, with plate
„ 52 „ between bilge keel holes „ „ „
„ 53 „ „ holes of upper landing, rivets drilled out
„ 54 „ drilled and treated similar to 53
„ 55 „ adjoining holes in upper landing with the plate
„ 56 Bend (with the plate) near centre
„ 57 „ „ „ „ „ „
„ 58 Tensile between holes of butt (close to upper landing), rivets drilled out
„ 59 „ drilled and treated similar to 58

Plate F 6.

Aft end of Plate.	No.	60	Tensile between holes of upper landing, rivets cut out
	„	61	„ punched and treated same as 60
	„	62	„ with the plate
	„	63	Bend „ „ „ adjoining 62
	„	64	„ „ „ „ „ „
	„	65	Tensile „ „ „ continuation of 64
	„	66	Bend „ „ „
	„	67	Tensile between holes of butt
	„	68	„ punched same as 67
	„	69	„ through the inner holes of lower landing, rivets drilled out
	„	70	„ drilled and treated same as 69
	„	71	„ adjoining upper landing where 69 is
Centre of Plate.	„	72	Tensile, centre part of plate
	„	73	Bend „ „
	„	74	„ „ „
	„	75	Tensile „ „
Fore end of Plate.	„	76	Tensile between holes of lower landing, rivets cut out
	„	77	„ punched and treated same as 76
	„	78	„ adjoining 76
	„	79	Bend adjoining 77 with the plate
	„	80	„ „ „ „
	„	81	Tensile near centre, and with the plate
	„	82	Bend across plate close to frame holes
	„	83	„ „ „ „ „ „
	„	84	Tensile through outer row of holes in upper landing, rivets cut out
	„	85	„ punched and tested same as 84
	„	86	„ adjoining landing where 84 is from
	„	87	„ through outer row of holes in butt in upper landing
	„	88	„ punched and treated same as 87
	„	89	„ adjoining butt where 87 is from

Plate F 7.

Aft end of Plate.	„	90	Tensile between holes of upper landing, rivets cut out
	„	91	„ punched and treated same as 90
	„	92	„ adjoining landing where 90 is from

PLATE F 7—*continued.*

Aft end of Plate.	No. 93	Bend with the plate
	„ 94	„ „ „ „
	„ 95	Tensile between holes of butt near lower landing
	„ 96	„ punched and treated same as 95 (across plate)
	„ 97	„ through holes of frame
	„ 98	„ punched and treated same as 97
	„ 99	„ adjoining frame at 97 test
	„ 100	„ „ butt „ 95 „
Centre of Plate.	„ 101	Tensile from centre of plate
	„ 102	Bend „ „ „
	„ 103	„ diagonally „ „
	„ 104	Tensile „ „ „
	„ 105	„ across „ „ „
Fore end of Plate.	„ 106	Tensile between holes of lower landing (rivets drilled out)
	„ 107	„ drilled and tested similar to 106
	„ 108	„ adjoining landing where 106 is from
	„ 109	Bend with plate adjoining 107
	„ 110	„ „ „ „ „
	„ 111	Tensile between holes in butt
	„ 112	„ punched and treated similar to 111
	„ 113	„ adjoining butt where 111 is from (across plate)
	„ 114	„ between holes of upper landing (rivets cut out)
	„ 115	„ punched and treated same as 114
	„ 116	„ adjoining 114

BUTTSTRAP BETWEEN PLATES E 5 AND 6.

	„ 117	Tensile through row of holes fore and aft
	„ 118	„ between holes fore and aft
	„ 119	„ „ „ up and down
	„ 120	„ through row of holes up and down

BUTTSTRAP BETWEEN PLATES F 6 AND 7.

	„ 121	Tensile through row of holes up and down
	„ 122	„ between holes „
	„ 123	Bend „ „ „
	„ 124	Tensile „ „ „

TABLE VI.

Results of Tests from Plates taken from damaged steel steamer. (*See* Plates XIII. and XIV.)

PIECES FROM SOLID PART OF PLATE.

No. of Sample.	Dimensions.		Area.	Tons on area.	Tons on sq. in.	Elongation.		Remarks.
	Width.	Thickness.				Actual.	Per cent. on 8 ins.	
	in.	in.	sq. in.			ins.		
52	1·48	·555	·821	23·04	28·0	$1\frac{1}{32}\frac{7}{2}$	19·0	Sample slightly pitted.
101	1·49	·46	·686	19·15	27·9	$1\frac{1}{16}\frac{5}{0}$	24·0	〃 〃 〃
55	〃	·545	·812	23·12	28·5	$2\frac{1}{8}$	26·5	〃 〃 〃
65	〃	·465	·693	19·8	28·6	$2\frac{1}{8}$	26·0	
92	〃	·48	·716	21·13	29·5	$1\frac{1}{16}\frac{3}{0}$	23·0	
99	〃	·47	·701	19·71	28·1	$1\frac{3}{4}$	22·0	
48	1·485	·56	·832	23·9	28·7	$1\frac{7}{8}$	23·5	
23	1·49	·455	·678	18·97	28·0	$2\frac{1}{32}\frac{5}{2}$	31·0	
44	1·5	·57	·856	24·16	28·2	$1\frac{7}{8}$	23·5	
89	1·495	·455	·68	19·51	28·7	$1\frac{1}{16}\frac{5}{0}$	24·0	
18	1·485	〃	·676	19·24	28·4	$1\frac{3}{4}$	22·0	Sample nicked on surface.
100	1·495	·465	·696	18·94	27·2	$1\frac{1}{16}\frac{3}{0}$	22·5	Sample pitted.
15	〃	·46	·688	18·33	26·7	$1\frac{1}{4}$	15·5	〃 〃
14	〃	〃	〃	19·46	28·3	2 Bare	24·5	〃 〃
104	〃	·465	·696	20·07	28·8	$2\frac{3}{16}$	27·0	
10	1·25	·475	·594	17·26	29·1	$1\frac{1}{16}\frac{5}{0}$	24·0	
43	1·495	·56	·838	23·88	28·5	$1\frac{7}{8}$	23·5	
4	1·485	·465	·691	20·08	29·0	$2\frac{1}{8}$	26·5	
2	〃	〃	〃	19·66	28·4	$1\frac{7}{8}$	23·5	〃 〃
113	1·495	·47	·703	20·5	29·2	$1\frac{1}{16}\frac{3}{0}$	22·5	
39	〃	·55	·823	21·98	26·7	2 Bare	24·5	
27	1·49	·45	·67	18·91	28·2	$1\frac{7}{8}$	23·5	〃 〃
116	1·5	·48	·72	21·34	29·6	2	25·0	
105	1·485	·46	·683	19·5	28·5	$1\frac{7}{16}$	19·0	〃 〃
51	〃	·55	·817	22·14	27·1	$1\frac{2}{32}\frac{1}{2}$	20·5	
71	1·495	·455	·68	19·65	28·9	$2\frac{1}{8}$	26·5	
32	1·485	·545	·81	23·72	29·3	$1\frac{7}{8}$	23·5	
75	1·495	·47	·703	20·38	29·0	$1\frac{1}{16}\frac{3}{0}$	23·0	
122	1·56	·455	·71	20·91	29·4	$2\frac{3}{8}$	29·5	
72	1·495	·48	·718	20·23	28·2	$2\frac{3}{16}$	27·0	
62	〃	·455	·68	19·71	29·0	2	25·0	
78	〃	·46	·688	19·82	28·8	$2\frac{1}{8}$ Bare.	26·0	
108	〃	·45	·673	19·58	29·1	$2\frac{3}{16}$	27·0	〃 〃
86	〃	·45	〃	19·33	28·7	$2\frac{1}{8}$	26·5	
81	〃	·46	·688	20·17	29·3	$2\frac{1}{8}$ Bare.	26·0	
118	1·56	·615	·96	27·72	28·9	2	25·0	
119	〃	〃	〃	27·92	29·1	$1\frac{1}{16}\frac{5}{0}$	24·0	

PIECES TAKEN BETWEEN HOLES.

No. of Sample.	Dimensions.		Area.	Tons on area.	Tons on sq. in.	Elongation.		Remarks.
	Width.	Thickness.				Actual.	Per cent. on 8 ins.	
	in.	in.	sq. in.			ins.		
114	·795	·475	·3776	11·14	29·5	$1\frac{3}{4}$	22·0	Sample pitted.
115	·8	〃	·38	11·33	29·8	$1\frac{15}{16}$	24·0	
90	·67	·46	·3082	9·24	29·9	$1\frac{1}{2}$	19·0	〃 〃
91	〃	·47	·3149	9·36	29·7	$1\frac{11}{16}$	21·0	〃 〃
46	·69	·55	·3795	11·16	29·4	$1\frac{1}{2}$	19·0	
47	〃	·56	·3864	11·11	28·7	$1\frac{1}{2}$ Bare.	18·5	
17	·705	·45	·3172	9·09	28·6	$1\frac{5}{16}$	16·5	〃 〃
19	·7	·445	·3115	8·79	28·2	$1\frac{13}{32}$	17·5	〃 〃
28	·75	·55	·4125	12·2	29·5	$1\frac{19}{32}$	20·0	
29	〃	〃	〃	12·13	29·4	$1\frac{17}{32}$	19·0	〃 〃
53	·585	·545	·3188	8·97	28·1	$1\frac{5}{16}$	17·0	〃 〃
54	·62	·55	·341	9·75	28·5	$1\frac{23}{32}$	21·5	
30	·59	〃	·3245	9·33	28·7	$1\frac{5}{16}$	17·0	〃 〃
31	·6	〃	·33	9·62	29·1	$1\frac{5}{8}$	20·0	
95	·62	·46	·2852	7·79	27·3	$1\frac{19}{32}$	16·0	
96	·61	·45	·2745	7·87	28·6	$1\frac{3}{16}$	14·5	
106	·74	·46	·34	10·4	30·6	$2\frac{5}{32}$	26·5	
107	·75	·465	·3487	10·43	29·9	$1\frac{7}{8}$	23·5	〃 〃
111	·685	·48	·3288	10·22	31·0	$1\frac{1}{2}$	19·0	
112	·65	·465	·3022	9·0	29·7	$1\frac{7}{16}$	19·5	
22	·63	·46	·2898	8·14	28·0	$1\frac{23}{32}$	21·5	〃 〃
24	·625	·445	·2781	7·81	28·0	$1\frac{7}{8}$	23·0	
1	·655	·47	·3078	8·66	28·1	$1\frac{7}{16}$	19·5	〃 〃
3	·655	·47	·3078	8·79	28·5	$1\frac{15}{16}$	24·0	
58	·94	·545	·5123	14·61	28·5	$1\frac{1}{4}$	15·0	〃 〃
59	·94	·55	·517	14·62	28·2	$1\frac{7}{16}$	19·5	〃 〃
35	·67	·55	·3685	9·22	25·0	$1\frac{23}{32}$	21·5	〃 〃
36	·67	·555	·3718	10·75	28·9	$1\frac{11}{16}$	21·0	〃 〃
60	·79	·45	·355	10·1	28·4	$1\frac{3}{4}$	22·0	
61	·79	·45	·355	10·24	28·8	$1\frac{7}{8}$	23·0	
76	·79	·465	·367	10·43	28·4	$1\frac{9}{16}$	21·0	
77	·79	·465	·367	10·57	28·8	$1\frac{15}{16}$	24·0	
67	·78	·47	·366	10·27	28·1	$1\frac{3}{16}$	15·0	
68	·79	·47	·371	10·36	27·9	$1\frac{7}{16}$	18·5	

Pieces having holes punched through middle.

Note.—These results are only approximate as the Areas on each side of the holes varied owing to there having been more metal left on one side than on the other, and also to the holes not being perfectly round.

No. of Sample.	Dimensions (approximate).	Area (approximate).	Tons on area.	Tons on square inch.	Elongation.		Remarks.
					Actual.	Per cent. on 8 ins.	
	in.	sq. in.			in.		
69	·26 + ·235 × ·46	·228	7·65	33·59	$\frac{3}{16}$	2·5	
70	·285 + ·3 × ·465	·272	8·5	31·25	$\frac{11}{32}$	4·5	
37	·45 + ·21 × ·55	·363	9·8	27·0	—	—	No. 37, Could not obtain Elongation owing to piece not being straight after breaking.
38	·415 + ·215 × ·565	·356	10·47	29·4	$\frac{3}{16}$	2·5	
25	·38 + ·06 × ·44	·193	5·28	27·2	$\frac{5}{16}$	4·0	
26	·355 + ·175 × ·44	·233	6·65	28·5	$\frac{1}{8}$	1·5	
97	·28 + ·28 × ·47	·263	7·96	30·2	$\frac{1}{4}$ Bare	3·0	
98	·35 + ·26 × ·47	·287	8·06	28·1	$\frac{3}{32}$	1·0	
6	·28 + ·27 × ·47	·258	7·18	27·7	$\frac{1}{8}$	1·5	
7	·3 + ·12 × ·47	·197	6·39	32·01	$\frac{1}{4}$	3·0	
84	·315 + ·265 × ·455	·296	8·72	29·4	$\frac{1}{4}$	3·0	
85	·47 + ·165 × ·46	·332	7·47	24·5	$\frac{3}{32}$	1·0	
8	·21 + ·47 × ·48	·326	9·79	30·0	$\frac{15}{32}$	6·0	
9	·4 + ·23 × ·47	·295	8·58	29·0	$\frac{3}{16}$	2·5	
87	·18 + ·39 × ·465	·265	8·09	30·5	$\frac{1}{8}$	1·5	
88	·17 + ·46 × ·46	·290	7·44	25·6	$\frac{3}{32}$	1·0	
117	·24 + ·36 × ·62	·372	10·07	27·07	$\frac{1}{32}$	0·5	
120	·42 + ·19 × ·61	·372	11·1	29·8	$\frac{1}{32}$	0·5	
121	·32 + ·41 × ·455	·332	9·64	29·03	$\frac{5}{32}$	2·0	

The following were bent cold each piece standing the test, there being no failures :—

No. of Piece.	No. of Piece.	No. of Piece.
74	82	103
40	49	73
123	83	66
42	21	102
13	63	33
80	110	109
45	93	16
34	79	94
12	5	41
50	11	57
56	20	64

TABLE VII.

Analysis of the Foregoing Tests.

In the foregoing record the tensile tests, 90 in number are divided into three groups :—1st. Pieces from solid parts of the plates not in the immediate neighbourhood of rivet holes; 2nd. Pieces taken between holes; 3rd. Pieces having holes through the middle of them. The results of the 3rd group are not absolutely reliable on account of the impossibility of measuring exactly the sectional area exposed to stress.

The following shows for each group the highest, the lowest, and the mean of the breaking stress in tons per square inch and the extension per cent. on a length of 8 inches, except in the 3rd group where the extension is given in inches. In a piece with holes punched in the middle, the elongation takes place entirely at the sides of the holes. The extension should therefore be treated as an absolute quantity, and not as a per centage of the whole length.

		Highest.	Lowest.	Mean of all.
37 pieces from solid part of plate	Breaking stress	29·6	26·7	21·5
	Extension per cent.	31·0	15·5	24·2
34 pieces taken between holes	Breaking stress	31·0	25·0	28·8
	Extension per cent.	26·5	14·5	20·0
19 pieces having holes in them	Breaking stress	33·59	24·5	29·0
	Extension inches.	$\frac{15}{32}$ or ·47 in.	$\frac{1}{32}$ or ·03 in.	$\frac{3}{16}$ or ·19 in.

With 11 exceptions, all the 90 tests fulfilled the requirements that Lloyd's Register enforced at the time the vessel was built, viz., 27 to 31 tons tensile strength, and 16 per cent. extension.

THE FOLLOWING ARE THE EXCEPTIONS.

Test Number.	Breaking stress.	Extension.	
			FROM SOLID PART OF PLATE.
15	26·7	15·5 per cent.	Pitted sample, across grain from E 5, rolled hot, near centre
39	26·7	24·5 ,,	With grain from E 6, rolled cold, close to after butt and between holes of bilge fin. Rivets cut out.
			FROM PIECES TAKEN BETWEEN HOLES.
35	25·0	21·5 ,,	Badly pitted. With grain from E 6, rolled cold, between holes of lower landing close to after butt. Rivets cut out
58	28·5	15·0 ,,	Pitted, across grain from E 6, rolled cold, between holes of fore butt close to upper landing. Rivets drilled out.
67	28·1	15·0 ,,	Across grain from F 6, rolled cold, between holes of after butt. Rivets cut out.
96	28·6	14·5 ,,	Across grain from F 7, rolled hot and at black heat, between holes punched same as after butt.
			FROM PIECES HAVING HOLES THROUGH MIDDLE.
7	32·01	$\frac{1}{4}$ in.	Across grain from E 5, rolled hot, through outer holes of after butt. Rivets cut out.
69	33·59	$\frac{3}{16}$,,	With grain from F 6, rolled cold, through inner holes of lower landing. Rivets drilled.
70	31·25	$\frac{11}{32}$,,	With grain from F 6, rolled cold, through holes drilled same as inner holes of lower landing.
85	24·5	$\frac{3}{32}$,,	With grain from F 6, rolled cold, through holes punched same as outer holes of upper landing.
88	25·6	$\frac{3}{32}$,,	Across grain from F 6, rolled cold, through holes punched same as forward butt.

COMPARISON OF TENSILE STRENGTH AND EXTENSION.

I. Pieces taken between the rivet holes of landings or butts. II. Pieces taken between holes punched or drilled at the same spacing as the actual rivet holes. III. Pieces taken from the solid plate in an adjoining place, all lying the same way with or across the grain.

In each column there are two figures, 1st, the breaking stress per sq. inch; 2nd, the extension per cent. on 8 ins. The number of the test piece is put in brackets. Tests marked P. were pitted samples.

	Test piece between rivet holes.	Test between holes made for comparison.		Test from solid plate adjoining.		
	I.	II.	Excess I. over II. A.	III.	Excess I over III. B.	Excess II. over III. C.
		Holes	Punched.			
E. 5. Landing, rivets cut out	(17) P. 28·6 16·5	(19) P. 28·2 17·5	+ ·4 − 1·0	(18) 28·4 22·0	+ ·2 − 5·5	− ·2 − 4·5
,, ,, ,, ,,	(22) 28·0 21·5	(24) P. 28·0 23·0	0 − 1·5	(23) 28·0 31·0	0 − 9·5	0 − 8·0
E. 6. ,, ,, ,, *	(35) P. 25·0 21·5	(36) P. 28·9 21·0	− 3·9 + ·5	(39) 26·7 24·5	− 1·7 − 3·0	+ 2·2 − 3·5
,, ,, ,, ,,	(46) 29·4 19·0	(47) 28·7 18·5	+ ·7 + ·5	(48) 28·7 23·5	+ ·7 − 4·5	0 − 5·0
F 6 ,, ,, ,,	(60) 28·4 22·0	(61) 28·8 23·0	− ·4 − 1·0	(62) 29·0 25·0	− ·6 − 3·0	− ·2 − 2·0
,, ,, ,, ,,	(76) 28·4 21·0	(77) 28·8 24·0	− ·4 − 3·0	(78) 28·8 26·0	− ·4 − 5·0	0 − 2·0
F. 7. ,, ,, ,,	(90) P. 29·9 19·0	(91) P. 29·7 21·0	+ ·2 − 2·0	(92) 29·5 23·0	+ ·4 − 4·0	+ ·2 − 2·0
,, ,, ,, ,,	(114) P. 29·5 22·0	(115) 29·8 24·0	− ·3 − 2·0	(116) 29·6 25·0	− ·1 − 3·0	+ ·2 − 1·0
Mean			− ·46 − 1·2		− ·2 − 4·7	+ ·27 − 3·5
Mean, omitting 35, 36, 39 (with grain)			+ ·03 − 1·4		+ ·03 − 4·9	+ ·00 − 3·5
E. 6. Butt, rivets cut out .	(28) 29·5 20·0	(29) 29·4 19·0	+ ·1 + 1·0			
F. 6. ,, ,, ,,	(67) 28·1 15·0	(68 P.) 27·9 18·5	+ ·2 − 3·5			
F. 7. ,, ,, ,,	(95) 27·3 16·0	(96) 28·6 14·5	− 1·3 + 1·5	(100) 27·2 22·5	+ ·1 − 6·5	+ 1·4 − 8·0
,, ,, ,, ,,	(111) 31·0 19·0	(112) 29·7 19·5	+ 1·3 − ·5	(113) 29·2 22·5	+ 1·8 − 3·5	+ ·5 − 3·0
Mean (across the grain) .			+ ·07 − ·4		+ ·95 − 5·0	+ ·95 − 5.5
Mean of landings and butts with and across grain omitting 35, 36, 39			+ ·05 − 1·0		+ ·23 − 4·9	+ ·21 − 4·0
		Holes	Drilled.			
E. 5. Landing, rivets drilled	(1) P. 28·1 19·5	(3) 28·5 24·0	− ·4 − 4·5	(2) 28·4 23·5	− ·3 − 4·0	+ ·1 + ·5
E. 6. ,, ,, ,,	(30) P. 28·7 17·0	(31) 29·1 20·0	− ·4 − 3·0	(32) 29·3 23·5	− ·6 − 6·5	− ·2 − 3·5
,, ,, ,, ,,	(53) P. 28·1 17·0	(54) 28·5 21·5	− ·4 − 4·5	(55) 28·5 26·5	− ·4 − 9·5	0 − 5·0
F. 7. ,, ,, ,,	(106) 30·6 26·5	(107) P. 29·9 23·5	+ ·7 + 3·0	(108) 29·1 27·0	+ 1·5 − ·5	+ ·8 − 3·5
Mean			− ·12 − 2·2		+ ·05 − 5·1	+ ·17 − 2·9
E. 6. Butt rivets drilled .	(58) P. 28·5 15·0	(59) P 28·2 19·5	+ ·3 − 4·5			
Mean of landings and butts .	with and across grain		− ·04 − 2·7		+ ·05 − 5·1	+ ·17 − 2·9

* As No. 35 was badly pitted, it does not afford a fair comparison with Nos. 36 and 39. They have therefore been omitted.

COMPARISON OF TENSILE STRENGTH AND EXTENSION.

. Pieces taken between actual rivet holes. II. Pieces taken between holes punched or drilled at the same spacing. III. Pieces taken from the solid plate in an adjoining place.

PIECES TAKEN FROM AN INSIDE PLATE, THAT IS, THE PLATE NEXT RIVET "HEADS."

	Test piece between rivet holes. I.	Test between holes made for comparison. II.	Excess I. over II A.	Test from solid plate adjoining. III.	Excess I. over III. B.	Excess II. over III. C.
		PUNCHED	HOLES.			
	(60)	(61)		(62)		
F. 6. Landing, rivets cut out.	28·4 22·	28·8 23·	− ·4 − 1·	29·0 25·	− ·6 − 3·	− ·2 − 2·0
	(76)	(77)		(78)		
„ „ „	28·4 21·	28·8 24·	− ·4 − 3·	28·8 26·	− ·4 − 5·	— − 2·0
	(90) P.	(91)		(92)		
F. 7. „ „	29·9 19·	29·7 21·	+ ·2 − 2·	29·5 23·	+ ·4 − 4·	+ ·2 − 2·0
	(114) P.	(115)		(116)		
„ „ „	29·5 22·	29·8 24·	− ·3 − 2·	29·6 25·	− ·1 − 3·	+ ·2 − 1·0
Mean . .			− ·22 − 2·		− ·17 − 3·7	+ ·05 − 1·7
		DRILLED	HOLES.			
	(106)	(107) P.		(108)		
F. 7. Landing, rivets drilled	30·6 26·5	29·9 23·5	+ ·7 + 3·0	29·1 27·0	+ 1·5 − ·5	+ ·8 − 3·5

PIECES TAKEN FROM AN OUTSIDE PLATE, THAT IS, THE PLATE NEXT RIVET "POINTS."

		PUNCHED	HOLES.			
	(17) P.	(19) P.		(18)		
E. 5. Landing, rivets cut out	28·6 16·5	28·2 17·5	+ ·4 − 1·0	28·4 22·	+ ·2 − 5·5	− ·2 − 4·5
	(22)	(24)		(23)		
„ „ „	28·0 21·5	28·0 23·	0 − 1·5	28·0 31·	0 − 9·5	0 − 8·0
	(35) P.	(36) P.		(39)		
E. 6. „ „	25·0 21·5	28·9 21·	− 3·9 + ·5	26·7 24·5	− 1·7 − 3·0	+ 2·2 − 3·5
	(46)	(47)		(48)		
„ „ „	29·4 19·	28·7 18·5	+ ·7 + ·5	28·7 23·5	+ ·7 − 4·5	0 − 5·0
	(28)	(29)				
„ Butt, „ „	29·5 20·	29·4 19·	+ ·1 + 1·0			
	(67)	(68) P.				
F 6. „ „	28·1 15·	27·9 18·5	+ ·2 − 3·5			
	(95)	(96)				
F 7. „ „	27·3 16·	28·6 14·5	− 1·3 + 1·5			
	(111)	(112)				
„ „ „	31·0 19·	29·7 19·5	+ 1·3 − ·5			
Mean			− ·31 − ·4		− ·2 − 5·6	+ ·5 − 5·25
Mean, omitting 35, 36, 39 .			+ ·2 + ·1		+ ·3 − 6·5	− ·07 − 5·8
		DRILLED	HOLES.			
	(1) P.	(3)		(2)		
E. 5. Landing, rivets drilled	28·1 19·5	28·5 24·	− ·4 − 4·5	28·4 23·5	− ·3 − 4·0	+ ·1 + ·5
	(30) P.	(31)		(32)		
E. 6. „ „ „	28·7 17·0	29·1 20·	− ·4 − 3·0	29·3 23·5	− ·6 − 6·5	− ·2 − 3·5
	(53) P.	(54)		(55)		
„ „ „ „	28·1 17·0	28·5 21·5	− ·4 − 4·5	28·5 26·5	− ·4 − 9·5	0 − 5·0
	(58) P.	(59)				
„ Butt, „ „	28·5 15·0	28·2 19·5	+ ·3 − 4·5			
Mean			− ·22 − 4·1		− ·43 − 6·7	− ·33 − 2·7

COMPARISON OF TENSILE STRENGTH AND EXTENSION.

(I.) Pieces having through the middle actual rivet holes of landings or butts; (II.) Pieces having through the middle, holes similar to the actual rivet holes; (III.) Pieces taken from the solid plate in an adjoining place. All lying the same way with or across the grain.

In each column there are two figures, 1st the breaking stress per square inch, 2nd the extension in inches. The number of the test piece is put in brackets.

	Test piece with rivet holes through it. I.	Test pieces with holes punched for comparison. II.	Excess of I. over II. A.	Test from solid plate adjoining. III.	Excess of I. over III. B.	Excess of II. over III. C.
					BREAKING	STRESS.
	(8)	(9)		(10)		
E 5 bilge keel, rivets cut out	30·0 ·469	29·0 ·187	+1·0+·282	29·1 1·94	+·9	−·1
	(37)	(38)		(39)		
E 6 „ „ „	27·0	29·4 ·187	−2·4 −	26·7 2·0	+·3	+2·7
	(84)	(85)		(86)		
F 6 landing „ „	29·4 250	24·5 ·094	+4·9+·156	28·7 2·12	+·7	−4·2
Mean (with grain) . .			+1·2+·219		+·6	−·5
	(7)	(6)		(4)		
E 5 butt, rivets cut out .	32·0 ·250	27·7 ·125	+4·3+·125	29·0 2·12	+3·0	−1·3
	(25)	(26)		(27)		
„ „ „	27·2 ·312	28·5 ·125	−1·3+·187	28·2 1·87	−1·0	+·3
	(87)	(88)		(89)		
F 6 „ „	30·5 ·125	25·6 ·094	+4·9+·031	28·7 1·94	+1·8	−3·1
	(97)	(98)		(99)		
F 7 frame „ „	30·2 ·250	28·1 ·094	+2·1+·156	28·1 1·75	+2·1	0
Mean (across the grain) .			+2·5+·125		+1·5	−1·0
Mean (with and across grain)			+2·1+·166		+1·1	−·8
	(121)			(122)		
F butt strap rivets cut out	29·0 ·156			29·4 2·37	−·4	
	(117)			(118)		
E „ „ „	27·1 ·031			28·9 2·0	−1·8	
	(120)			(119)		
„ „ ., not known	29·8 ·031			29·1 1·94	+·7	
Mean . . .					−·5	
		DRILLED	HOLES.			
	(69)	(70)		(71)		
F 6 landing rivets drilled .	33·6 ·187	31·25 ·343	+2·35−·156	28·9 2·12	+4·7	+2·35

The following Table shows the variations in breaking stress and extension per cent. recorded for Pieces cut from the same plate. Pieces cut from the solid parts of the plates only are taken here. Those marked P. were "pitted."

Plate E 5.				Plate E 6.				Plate F 6.				Plate F 7.			
No.	Breaking Stress.	Extn.		No.	Breaking Stress.	Extn.		No.	Breaking Stress.	Extn.		No.	Breaking Stress.	Extn.	
2	28·4	23·5	P. with grain	32	29·3	23·5	with grain	62	29·0	25.0	with grain	92	29·5	23·0	with grain
10	29·1	24·0	,, ,,	39	26·7	24·5	,, ,,	65	28·6	26·0	,, ,,	101	27·9	24·0	P ,, ,,
18	28·4	22·0	with grain samp. nicked	44	28·2	23·5	,, ,,	71	28·9	26·5	,, ,,	108	29·1	27·0	P ,, ,,
23	28·0	31·0	with grain	48	28·7	23·5	,, ,,	72	28·2	27·0	,, ,,	116	29·6	25·0	,, ,,
Mean	28·48	25·1	,, ,,	51	27·1	20·5	,, ,,	78	28·8	26·0	,, ,,	Mean	29·0	24·75	,, ,,
				52	28·0	19·0	P ,, ,,	81	29·3	26·0	,, ,,				
4	29·0	26·5	across grain	55	28·5	26·5	P ,, ,,	86	28·7	26·5	,, ,,				
15	26·7	15·5	P ,, ,,	Mean	28·07	23·0	,, ,,	Mean	28·8	26·15	,, ,,	99	28·1	22·0	across grain
27	28·2	23·5	P ,, ,,									100	27·2	22·5	P ,, ,,
Mean	27·97	21·9	,, ,,	43	28·5	23·5	across grain	75	29·0	23·0	across grain	105	28·5	19·0	P ,, ,,
								89	28·7	24·0	,, ,,	113	29·2	22·5	,, ,,
14	28·3	24·5	P diagonal					Mean	28·85	23·5	,, ,,	Mean	28·25	21·5	,, ,,
												104	28·8	27·0	diagonal
Mean of all	28·26	23·8			28·12	23·06			28·8	25·55			28·65	23·55	

The thickness of the plates is not uniform, as may be seen from the variation in thickness of the test pieces taken from the same plate.

Thus pieces from E 5 vary from ·48in. (No. 8) to ·44in. (Nos. 25, 26).
„ „ E 6 „ „ ·57in. (No 44) to ·545in. (Nos. 32, 53, 55 and 58).
„ „ F 6 „ „ ·48in. (No. 72) to ·45in. (Nos. 60, 61 and 86).
„ „ F 7 „ „ ·48in. (Nos. 92, 111, 116) to ·45in. (Nos. 98 and 108).

The analysis (pages 105 and 106) shows two comparisons: 1st, between pieces taken from between actual rivet holes, the rivets being *cut out*, pieces taken from between similar holes *punched in an adjoining part of the plate*, and pieces taken from a solid adjoining part of the plate not in the immediate neighbourhood of rivet holes; 2nd, between pieces taken from between actual rivet holes, the rivets being *drilled out*, pieces taken from between similar holes *drilled in an adjoining part of the plate*, and pieces taken from a solid adjoining part of the plate. The first set are sub-divided into pieces with the grain and pieces across the grain.

The results show almost the same strength all through, there being a slight gain of the pieces between holes over the pieces from the solid. Looking to the differences of extension, the column B, page 105, comparing pieces between actual rivet holes with solid pieces, shows, in each case, a loss of extension per centage of about 5. This loss is probably to be attributed to the effect of punching holes, because in the two cases of punched experimental holes there is a corresponding loss, as shown by column C, of 3·5 and 5·5, or, taking the mean of all, of 4; while column A shows a difference of only 1 in percentage of extension between the pieces from between actual rivet holes and pieces from between experimentally made holes. This slight loss is easily explained by the rough treatment to which the material surrounding the holes has been subjected; first by the original punching, then by use of a drift to bring the holes fair, then by riveting, then by caulking, and finally by the cutting out of the rivets. It can only be a matter of surprise that tests taken from between the rivet holes do not show a greater loss of strength and ductility. In the analysis on page 106 the results are again divided into two groups, viz.: 1st, those from plate next rivet head; 2nd, those from plate next rivet point or countersink. It will be seen that there is not much difference between the two.

The analysis on page 107 shows comparisons between pieces with actual rivet holes through them, pieces with experimentally made holes through them, and pieces taken from the solid adjoining part of the plate. These experiments show no sign of injury in the material about the rivet holes. Column B, comparing the strength of pieces with actual rivet holes, and solid pieces shows on the whole greater strength in the former; while column A, comparing pieces with actual rivet holes and pieces with experimentally punched holes, shows an excess both of strength and of ductility in the former. The only example of experimentally drilled holes shows that the piece with actual rivet holes is stronger, but has less ductility than the piece with experimental holes. This loss of ductility is a result that might have been anticipated.

TABLE VIII.

EXPERIMENTS ON RIVETING.

Six pairs of ½-inch plates in all were experimented with, punched, as shown by Plate XV., holes being punched to represent in size and spacing double riveted landings in shell plating. Each pair after riveting was caulked round the edge, was then washed with sal-ammoniac, and was then immersed in the tidal dock, Leven Shipyard, so as to produce rusting in any flaws in the rivets. All the rivets were tested with testing hammer and found sound.

The rivets used were: iron rivets obtained from four different ship yards, being the rivets in every day use in those yards, marked in these experiments *a*, *b*, *c*, *d*; and steel rivets in everyday use in Leven Shipyard.

The riveting was done as follows.—

Plates DD., steel, 1st, iron rivets, class *a*.
2nd, steel rivets.
3rd, iron and steel in pairs.
Plates E., steel, 1st, iron rivets, class *b*.
2nd, steel rivets.
3rd, iron and steel in pairs.
Plates EE., steel, 1st, iron rivets, class *b*.
2nd, steel rivets.
3rd, iron and steel rivets, in pairs along one side and half another, the remaining holes being left empty.
Plates M., steel, 1st, iron rivets, classes *c*. and *d*.
2nd, steel rivets.
3rd, iron and steel rivets in pairs.
Plates I., iron, iron rivets in pairs, classes *a*, *b*, *c*, *d*.
Plates A., steel, iron rivets with differences in countersinking.

First Series.

Iron rivets cut from steel plates, 13th February, 1886.

Class of Rivets.	*a*.	*b*.	*c*.	*d*.
Plate	DD.	E. & EE.	M.	M.
Rivets cut out	48	88	34	14
,, broken at point ...	...	51	21	7
,, ,, head ...	...	37	13	7
,, considered bad ...	24	22	20	8
Percentage ,, ...	50 per cent.	25 per cent.	59 per cent.	57 per cent.

Second Series.

The same four steel plates DD., E., EE., and M., riveted up on 16th February, 1886, almost entirely with steel rivets, five iron rivets being placed among them. Rivets cut out 3rd March, 1886.

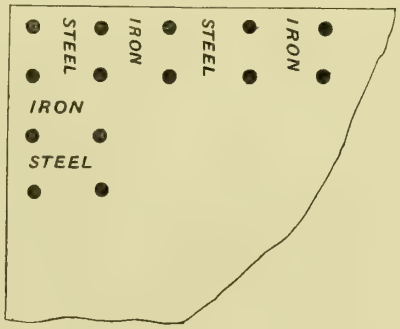
All five iron rivets broke at countersink, four of them showing bad fracture.

All the steel rivets sheared off at head after standing about four times as many blows as the iron ones.

Third Series.

In these experiments the steel rivets were placed side by side with the iron rivets as shown in sketch A. After being riveted up the heads of the rivets were stamped with a distinguishing letter, so that no confusion might arise as rivets were cut out.

SKETCH A



Each rivet as it was cut out was examined and the fracture noted at the time. The steel rivets on an average took more than three times the number of blows with an eight pound hammer to cut them out that the iron rivets did. In the case of bad rivets, which were all iron, one blow or two was sufficient to break them out, the fracture being quite black round the edge and sometimes well in towards the centre of the rivet.

The plates were riveted 5th March, 1886. Rivets cut out 19th March, 1886.

Steel and Iron Rivets in pairs cut from Steel Plate.

Plates E.

	Class *a.*		Class *b.*		Class *c.*		Class *d.*	
	Steel.	Iron.	Steel.	Iron.	Steel.	Iron.	Steel.	Iron.
Rivets cut out . . .	8	8	10	10	2	2	2	2
„ broken at point .	—	4	—	6	—	1	—	1
„ broken at head . .	8	4	10	4	2	1	2	1
„ considered bad . .	—	1	—	6	—	1	—	1
Percentage „ . .	—	12 per ct.	—	60 per ct.	—	50 per ct.	—	50 per ct.

PLATES D D.

	Steel.	*a.* Iron	Steel.	*b.* Iron.	Steel.	*c.* Iron.	Steel.	*d.* Iron.
Rivets cut out . . .	6	6	6	6	6	6	6	6
,, broken at point . .	—	2	—	5	—	3	—	6
,, broken at head . .	6	4	6	1	6	3	6	—
,, considered bad . .	—	3	—	5	—	3	—	5
Per centage ,, . .	—	50 per ct.	—	83 per ct.	—	50 per ct.	—	83 per ct.

PLATES M.

	Steel.	*a.* Iron.	Steel.	*b.* Iron.	Steel.	*c.* Iron.	Steel.	*d.* Iron.
Rivets cut out . . .	6	6	6	6	6	6	6	6
,, broken at point . .	—	4	—	3	—	4	1	3
,, broken at head . .	6	2	6	3	6	2	5	3
,, considered bad . .	—	1	—	1	—	3	—	2
Per centage ,, . .	—	16 per ct.	—	16 per ct.	—	50 per ct.	—	33 per ct.

PLATES E E.

	Steel.	*a.* Iron.	Steel.	*b.* Iron.	Steel.	*c.* Iron.	Steel.	*d.* Iron.
Rivets cut out . . .	6	4	4	4				
,, broken at point . .	1	3	1	3	Half	of plate	left unri	veted for
,, broken at head . .	5	1	3	1		testing	purposes.	
,, considered bad . .	—	3	—	3				
Per centage ,, . .	—	75 per ct.	—	75 per ct.				

4th Series. IRON PLATES WITH IRON RIVETS.

	a.	*b.*	*c.*	*d.*
	Iron.	Iron.	Iron.	Iron.
Rivets cut out . .	22	8	6	8
„ broken at point .	12	6	3	7
„ „ „ head .	10	2	3	1
„ considered bad .	8	5	2	5
Per centage „ .	36 per cent.	62 per cent.	33 per cent.	62 per cent.

The rivets in the above plate were $\frac{3}{4}$ in diameter, total number 44. This plate was submerged in dock on 15th March, and taken out on 19th March.

5th Series. STEEL PLATES A. IRON RIVETS WITH DIFFERENT COUNTERSINKS.

	A.	B.	C.	D
Rivets cut out . .	12	12	12	12
„ broken at point .	3	6	5	8
„ „ „ head .	9	6	7	4
„ considered bad .	5	6	2	4
Per centage „ .	42 per cent.	50 per cent.	17 per cent.	33 per cent.

Note.—A represents holes left as punched.
B „ „ countersunk 40° quite through.
C „ „ „ „ $\frac{2}{3}$ rds, „
D „ „ „ 24° quite „

This plate was submerged in dock on 9th March, was taken up and rivets cut out on 19th March, 1886.

TABLE IX.

Tests from Riveted Plates (Steel). *See* Plate XV.

Comparison of tensile strength and extension; (I.) Pieces taken between the rivet holes in the plates; (II.) Pieces taken between holes punched, at the same spacing as the actual rivet holes, in the solid part of the plate; (III.) Pieces taken from the solid part of the plate in the centre.

In each column there are two figures; 1st, the breaking stress per square inch; and 2nd, the extension per cent. on 8 inches. The number of the test piece, referring to diagrams, is put in brackets.

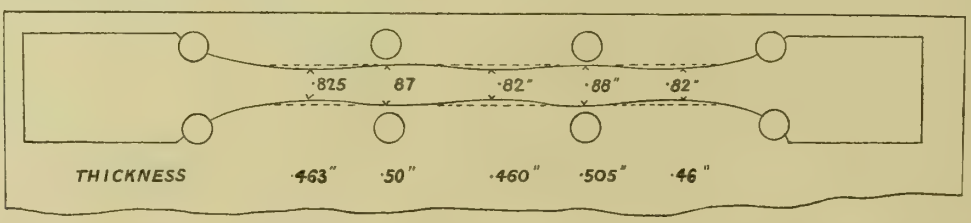
Description of Plate.	Test between actual rivet holes. I.	Test between holes made for comparison. II.	Excess. I. over II.	Test from solid plate. III.	Excess. I. over III.	Excess. II. over III.
	(1)	(2)		(3)		
AA. Riveted once, . heads . .	30·1, 23·0	29·5, 23·5	+ ·6, − ·5	29·9, 24·5	+ ·2, − 1·5	− ·4, − 1·0
	* (4)	(5)		(6)		
,, ,, . countersinks	30·2, 15·5	30·1, 21·5	+ ·1, − 6·0	30·0, 25·5	+ ·2, − 10·0	+ ·1, − 4·0
	(7)	(9)		(10)		
EE. Riveted twice, . heads . .	30·0, 22·0	30·2, 24·0	− ·2, − 2·0	30·4, 25·0	− ·4, − 3·0	− ·2, − 1·0
	(11)	(13)		(14)		
,, ,, . countersinks	30·4, 13·5	30·6, 19·5	− ·2, − 6·0	30·4, 24·5	0, − 11·0	+ ·2, − 5·0
	(8)	(9)		(10)		
EE. Riveted three times, heads . .	30·5, 20 0	30·2, 24·0	+ ·3, − 4·0	30·4, 25·0	+ ·1, − 5·0	− ·2, − 1·0
	§ (12)	(13)		(14)		
,, ,, ,, countersinks	30·0, 11·0	30·6, 19·5	− ·6, − 8·5	30·4, 24·5	− ·4, − 13·5	+ ·2, − 5·0
	† (5)			(6)		
M. Riveted three times, heads . .	32·7, 14·0	...	...	31·7, 18·0	+ 1·0, − 4·0	
	† (7)			‡ (8)		
,, ,, countersinks	32·4, 18·5	...	...	30·9, 18·0	+ 1·5, + ·5	
	(1)			(2)		
,, ,, ,, annealed, heads	30·0, 18·5	...	...	30·4, 21·0	− ·4, − 2·5	
	(3)			(4)		
,, ,, ,, ,, countersinks	30·6, 19·0	...	...	30·4, 25·0	+ ·2, − 6·0	

* AA 4 broke at mark of riveting hammer.

† M 5 & 7, test pieces not parallel.

‡ M 8 broke at flaw in surface.

§ EE 12 not broken so as to show inequality of extension. *See sketch.*



Tests, similar to those from Plate A, were to have been made from iron plate, but the pieces broke across when being sheared.

TABLE X.

Results of Tests Showing Effect of Galvanizing on Iron and Steel.

Tests made at Parkhead Iron and Steel Works, Oct. 20th, 1884.

STEEL.

No. of test piece.	1.	2.	3.	4.	5.	6.	7.	8.
Breadth of piece	1·28	1·31	1·28	1·29	1·29	1·28	1·29	1·28
Thickness	·18	·19	·25	·32	·16	·18	·23	·33
	$\frac{5}{32}$	$\frac{6}{32}$	$\frac{8}{32}$	$\frac{11}{32}$	$\frac{5}{32}$	$\frac{6}{32}$	$\frac{8}{32}$	$\frac{11}{32}$
Area of section in square inch	·23	·248	·32	·413	·206	·23	·296	·422
Breaking strain in tons ungalvanized	7·3	6·18	10·28	11·3	6·3	6·95	9·64	12·14
,, ,, ,, galvanized	7·6	6·51	9·43	11·75	6·38	7·19	9·59	12·45
Elongation in inches ungalvanized	·64	2·	1·44	1·92	1·56	1·68	1·12	2·04
,, ,, galvanized	·84	1·48	1·	1·52	1·52	1·25	1·12	1·56
Breaking strain per square inch ungalvanized	31·7	24·9	32·1	27·3	30·5	30·2	32·5	28·7
,, ,, ,, ,, galvanized	33·0	26·2	29·4	28·4	31·3	31·2	32·4	29·5
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Percentage of extension ungalvanized	*8	25	18	24	$19\frac{1}{2}$	21	14	$25\frac{1}{2}$
,, ,, galvanized	$10\frac{1}{2}$	$18\frac{1}{2}$	$12\frac{1}{2}$	19	19	$15\frac{1}{2}$	14	17

* Note.—The small extension of this piece is due to the unequal thickness, being $\frac{5}{32}$ inch at one end and $\frac{6}{32}$ inch at the other.

Nos. 6 and 7 ungalvanized and galvanized were not quite parallel, and therefore lose in extension.

ANALYSIS.

Breaking strain per square inch.		Confirmation of increase of breaking strain, due to galvanizing.
Ungalvanized.	Galvanized.	
31·7	33·	1
24·9	26·2	1
32·1	29·4	0
27·3	28·4	1
30·5	31·3	1
30·2	31·2	1
32·5	32·4	0
28·7	29·5	1

Description.	Mean breaking strain per square inch.
Galvanized	30·175
Ungalvanized	29·8
Mean difference	·375
Confirmation of gain of breaking strain	6

Percentage of Extension.		Confirmation of loss of Extension.
Ungalvanized.	Galvanized.	
8	10·5	0
25	18·5	1
18	12·5	1
24	19·	1
19·5	19·	1
21·	15·5	1
14·	14·	1
25·5	17·	1

Description.	Mean per cent. of extension.
Ungalvanized	19·375
Galvanized	15·75
Mean difference	3·625
Confirmation of loss of extension	7

TABLE X—*continued.*

IRON. Nov. 6TH, 1884.

No. of test piece.	*1	*2.	*3.	*4.	5.	6.	*7.	8.
Breadth of piece	1·25	1·27	1·28	1·29	1·25	1·26	1·27	1·28
Thickness „	·15	·18	·24	·298	·17	·21	·25	·3
Area of section in square inches	·187	·225	·307	·384	·212	·264	·317	·384
Breaking strain in tons ungalvanized.	3·76	5·09	5·7	8·74	4·2	5·73	7·46	9·49
„ „ „ galvanized	3·22	5·35	5·5	6·5	4·0	5·55	5·98	8·9
Elongation in inches ungalvanized	·08	·32	·08	·16	·4	·24	·32	·4
„ „ galvanized.	·06	·48	·00	·08	·16	·24	·16	·24
Breaking strain per square inch ungalvanized	20·1	22·3	18·5	22·7	19·8	21·7	23·5	24·7
„ „ „ galvanized	17·2	23·7	18·0	16·9	18·8	20·7	18·8	23·1
Percentage of extension ungalvanized	Per cent. 1	Per cent. 4	Per cent. 1	Per cent. 2	Per cent. 5	Per cent. 3	Per cent. 4	Per cent. 5
„ „ galvanized	·075	6	0	1	2	3	2	3

*NOTE.—Test pieces Nos. 1, 2 and 3 were rough on the surface. Nos. 4 and 7 were slightly laminated.

Breaking strain per square inch.		Confirmation of loss of breaking strain due to galvanizing.
Ungalvanized.	Galvanized.	
20·1	17·2	1
22·3	23·7	0
18·5	18·0	1
22·7	16 9	1
19·8	18·8	1
21·7	20·7	1
23·5	18·8	1
24·7	23·1	1

Description.	Main breaking strain per square inch.
Galvanized	19·65
Ungalvanized	21·66
Mean difference	2·01
Confirmation of loss of breaking strain	7

Percentage of extension on 8 inch.		Confirmation of loss of extension due to galvanizing.
Ungalvanized.	Galvanized.	
per cent.	per cent.	
1	·075	1
4	6	0
1	0	1
2	1	1
5	2	1
3	3	0
4	2	1
5	3	1

Description.	Mean per cent. of extension on 8 inch.
Galvanized	2·134
Ungalvanized	3·125
Mean difference	·991
Confirmation of loss of extension	6

SUMMARY OF COMPARATIVE RESULTS OF TESTS, GALVANIZED AND UNGALVANIZED IN IRON AND STEEL, MADE AT PARKHEAD IRON AND STEEL WORKS, OCT. 20TH AND NOV. 6TH, 1884.

		Steel.	Iron.
A	Mean breaking strain per square inch, ungalvanized . . .	29·8	21·6
	„ „ „ „ galvanized	30·175	19·65
A	Ratio of galvanized to ungalvanized breaking strain . . .	1·01	·907
B	Mean per centage of extension, ungalvanized	19·375	3·125
	„ „ „ galvanized	15·75	2·134
B	Ratio of galvanized to ungalvanized extension, per cent. . . .	·813	·683
	Confirmation of increase of breaking strain due to galvanizing . .	6 in 8	...
	„ decrease „ „ „ „ . .	...	7 in 8
	„ loss of extension due to galvanizing . . .	7 in 8	6 in 8

NOTE.—The steel plates, 1, 2, 3, 4, used in the above experiments, were taken from stock in Leven Shipyard (of Parkhead manufacture), and were cut in the following order, viz., two pieces cut from same plate (side by side), one piece being tested in the usual way against the other piece galvanized. Four thicknesses were chosen, viz., $\frac{5}{32}$ in., $\frac{6}{32}$ in., $\frac{8}{32}$ in., $\frac{11}{32}$ in. A similar set of duplicate pieces were taken from other plates in stock of same thickness, but of Blochairn manufacture, numbered 5, 6, 7, and 8 consecutively, making sixteen pieces in all, eight of which were galvanized and tested against eight ungalvanized. Iron plates were chosen from stock in yard, and tested in a similar manner to the above.

TABLE XI.

RESULTS OF TESTS SHOWING EFFECT OF GALVANIZING UPON STEEL PLATES MADE AT BLOCHAIRN STEEL WORKS, 16TH MARCH, 1886.

PLATE.	Original.		Pickled.		Galvanized.	
	Breaking stress per sq. in.	Elongation.	Breaking stress per sq. in.	Elongation.	Breaking stress per sq. in.	Elongation.
$\frac{5}{8}$ in. 8907	31·0	23·0	30·34	23	31·8	19·5
,, ,,	30·9	24·0	30·28	23·5	31·8	21·5
Mean.	30·95	23·5	30·31	23·25	31·8	20·5
Gain or Loss.			−·64	−·25	+·85	−3·0
$\frac{5}{8}$ in. 2143	28·6	21·0	28·15	25 0	29·8	19·5
,, ,,	29·1	21·5	28·91	24·5	29·7	23·0
Mean.	28·85	22·75	28·53	24·75	29·75	21·25
Gain or Loss.			−·32	+2·00	+·90	−1·50
$\frac{1}{2}$ in 9289	33·0	16·0	30·74	21·5	32·9	23·5
,, ,,	32·8	17·0	32·42	16 0	31·7	17·0
Mean.	32·9	16·5	31·58	18·75	32·3	20 25
Gain or Loss.			−1·32	+2·25	−·6	+3 75
$\frac{1}{2}$ in. 1796	29 0	22·0	27·53	26	28·6	23
,, ,,	27·7	27 0	27·76	27·5	29·0	24·5
Mean.	28·35	24·5	27·64	26·75	28·8	23·75
Gain or Loss.			−·71	+2.25	+·45	−·75
$\frac{1}{4}$ in. 1740	32·4	19·5	33·81	17·0	32·6	21 0
,, ,,	34·0	21·0	33·75	17·0	34·6	18·5
Mean.	33·2	20·25	33·78	17·0	33·6	19·75
Gain or Loss.			+·58	−3·25	+·4	−·50
$\frac{1}{4}$ in. 2480	27·2	23	26·97	20·0	26·7	22·0
,, ,,	26·7	24	27·37	19·5	27·0	19·0
Mean.	26·95	23·5	27·17	19·75	26·85	20·5
Gain or Loss.			+·22	−3·75	−·10	−3·0
$\frac{1}{8}$ in. 1388	31·2	22·5	31·2	23·0	30·7	14·0
,, ,,	31·4	21·0	31·1	20·0	31·0	20·0
Mean.	31·3	21·75	31·15	21·5	30·85	17·0
Gain or Loss.			−·15	−·25	−·45	−4·75
$\frac{1}{8}$ in. 1334	27·6	25·0	27·58	24·5	28·4	25·0
,, ,,	27·6	27·0	27·42	26·0	27·2	24·0
Mean.	27·6	26·0	27·50	25·25	27·8	24·5
Gain or Loss.			−·10	−·75	+·20	−1·5
$\frac{1}{16}$ in. 1389	32·0	19	30·92	21·0	29·9	10·5
,, ,,	30·7	18	30·14	16·5	31·1	15·5
Mean.	31·35	18·5	30·53	18·75	30·5	13·0
Gain or Loss.			−·82	+25	−·85	−5·5
$\frac{1}{16}$ in. 1335	25·3	20	28·74	17·0	27·0	17·0
,, ,,	26·7	17	26·35	20·0	26·8	20·0
Mean.	26·0	18·5	27·54	18·5	26·9	18·5
Gain or Loss.			+1·54	—	+·9	—

In addition to these tensile tests, 6 bend tests were made from each plate : 2 in original condition, 2 pickled and 2 galvanised. These were all bent cold, without tempering to the ordinary amount of bend ; that is to a curve whose radius is $1\frac{1}{2}$ times the thickness of the plate. These bends were all satisfactory, except No. 13, a galvanised bend from plate 9,289, which broke across when almost at the required curvature. The duplicate galvanised piece and the two pickled and two original pieces from this plate stood quite well.

The accompanying Table shows that galvanising does not produce much effect on the tensile strength of steel, and that it diminishes the extension slightly.

It appears that a distinction can be drawn between hard and soft steel, as follows :—

Breaking Stress.

Hard Steel.			Soft Steel.		
Plate No.	Gain in Galvanizing.	Loss in Galvanizing.	Plate No.	Gain in Galvanizing.	Loss in Galvanizing.
$\frac{5}{8}$ in. 8,907	·85	—	2,143	·90	—
$\frac{1}{2}$,, 9,289	—	·6	1,796	·45	—
$\frac{1}{4}$,, 1,740	·40	—	2,480	—	·10
$\frac{1}{8}$,, 1,388	—	·45	1,334	·20	—
$\frac{1}{16}$,, 1,389	—	·85	1,335	·90	—
	1·25	1·90		2·45	·10
	Mean	·13			
Mean	omitting 9,289	·01	Mean	·47	

Elongation per Cent.

Hard Steel.			Soft Steel.		
$\frac{5}{8}$ in. 8,907	—	3·0	2,143	—	1·50
$\frac{1}{2}$,, 9,289	3·75	—	1,796	—	·75
$\frac{1}{4}$,, 1,740	—	·5	2,480	—	3·00
$\frac{1}{8}$,, 1,388	—	4·75	1,334	—	1·5
$\frac{1}{16}$,, 1,389	—	5·5	1,335	—	—
	3·75	13·75			
	Mean	2·0		Mean	1·35
Mean	omitting 9,289	3·44			

No. 9,289 was not uniform in quality.

TABLE XII.

Table showing loss of weight in Pickling and gain in Galvanising of Pieces shown in Table XI.

WEIGHTS OF PIECES FOR TESTING BEFORE AND AFTER PICKLING.
WEIGHED AT BLOCHAIRN ON 16/3/86.

No.			Weight before Pickling (grams.)	Weight after Pickling (grams.)	Loss in Pickling.	Loss per cent.
$\frac{5}{8}$ inch	8907	A	3162·3	3154·3	8·0	·253
	,,	B	3159·8	3151·3	8·5	·269
	2143	A	3442·6	3437·5	5·1	·148
	,,	B	3373·7	3359·0	14·7	·436
$\frac{1}{2}$,,	9289	A	2422·1	2415·8	6·3	·260
	,,	B	2190·8	2185·0	5·8	·265
	1796	A	2265·0	2261·5	3·5	·154
	,,	B	2166·5	2163·5	3·0	·138
$\frac{1}{4}$,,	1740	A	1134·0	1126·0	8·0	·705
	,,	B	1104·8	1096·5	8·3	·751
	2480	A	1227·0	1216·0	11·0	·896
	,,	B	1136·8	1127·4	9·4	·827
$\frac{1}{8}$,,	1388	A	580·6	573·1	7·5	·129
	,,	B	578·9	572·1	6·8	·117
	1334	A	516·6	506·4	10·2	·197
	,,	B	549·4	539·9	9·5	·173
$\frac{1}{16}$,,	1389	A	289·0	281·2	7·8	·270
	,,	B	289·5	281·2	8·3	·287
	1335	A	289·3	281·5	7·8	·270
	,,	B	281·2	273·4	7·8	·277
						·341 Mean.

WEIGHT OF PIECES FOR TESTING BEFORE AND AFTER GALVANISING. WEIGHED at BLOCHAIRN, ON 16/3/86.

No.			Weight before Galvanising (grams).	Weight after Galvanising (grams).	Gain in Galvanising.	Gain per cent.	
$\frac{5}{8}$	8907	5	2715	2752	37	1·4	
,,	,,	6	2773	2803	30	1·1	1·25
,,	2143	2	2631	2663	32	1·2	
,,	,,	3	2677	2711	34	1·3	
$\frac{1}{2}$	9289	13	2662	2705	43	1·6	
,,	,,	14	2338	2369	31	1·3	1·4
,,	1796	10	2553	2582	29	1·1	
,,	,,	12	2269	2306	37	1·6	
$\frac{1}{4}$	1740	21	1138	1159	21	1·8	
,,	,,	22	1109	1146	37	3·3	1·97
,,	2480	17	1235	1254	19	1·5	
,,	,,	18	1255	1272	17	1·3	
$\frac{1}{8}$	1388	37	581·4	603·1	21·7	3 7	
,,	,,	38	556·2	575·8	19·6	3·5	3·0
,,	1334	29	542·3	557·0	14·7	2·7	
,,	,,	30	554·3	566·0	11·7	2·1	
$\frac{1}{16}$	1389	33	295·5	309·3	13·8	4·7	
,,	,,	34	290·0	308·5	18·5	6·4	5·75
,,	1335	25	280·7	299·0	18·3	6 4	
,,	,,	26	279·3	294·5	15·2	5·5	
						2·675 Mean.	

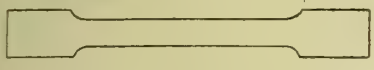
TABLE XIII.

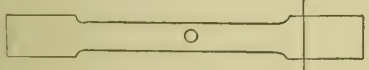
RESULT OF TESTS SHOWING EFFECTS OF GALVANISING, PUNCHING AND DRILLING, &C., UPON STEEL PLATES.

TESTS MADE AT HALLSIDE, 29TH MARCH, 1886.

(The holes in each case were $\frac{1}{2}$ inch. Breadth of test pieces about $1\frac{3}{8}$ inch.

Description.	Breaking stress per square inch.	Elongation in 8 inches.
		Per cent.
A. 0. $\frac{1}{2}$ inch, plain .	28·8	19·0
B. 0. $\frac{1}{2}$,, ,, .	30·3	22·0
C. 0. $\frac{1}{2}$,, ,, .	29·4	22·5
D. 0. $\frac{3}{8}$,, ,, .	28·9	18·5
E. 0. $\frac{3}{8}$,, ,, .	31·5	19·0
F. 0. $\frac{3}{8}$,, ,, .	28·7	16·5
Means . .	29·6	19·6
A. 1. $\frac{1}{2}$ inch galvanised .	29·5	16
B. 1. $\frac{1}{2}$,, ,, .	31·1	18
C. 1. $\frac{1}{2}$,, ,, .	30·6	22
D. 1. $\frac{3}{8}$,, ,, .	30·0	19
E. 1. $\frac{3}{8}$,, ,, .	31·9	18·5
F. 1. $\frac{3}{8}$,, ,, .	29·3	21
Means . .	30·4	19·1
		Inch.
A. 3. $\frac{1}{2}$ inch, punched plain	27·2	·20
B. 3. $\frac{1}{2}$,, ,, ,,	29·2	·16
C. 3. $\frac{1}{2}$,, ,, ,,	27·7	·12
D. 3. $\frac{3}{8}$,, ,, ,,	25·7	·16
E. 3. $\frac{3}{8}$,, ,, ,,	28·7	·12
F. 3. $\frac{3}{8}$,, ,, ,,	29·6	·12
Means . .	28·0	·15
A. 4. $\frac{1}{2}$ inch galvanised and punched . .	26·7	·16
B. 4. $\frac{1}{2}$,, ,, ,,	29·4	·16
C. 4. $\frac{1}{2}$,, ,, ,,	28·2	·04 cryst
D. 4. $\frac{3}{8}$,, ,, ,,	26·0	·12
E. 4. $\frac{3}{8}$,, ,, ,,	30·1	.12
F. 4. $\frac{3}{8}$,, ,, ,,	30·4	.12
Means . .	28·5	.12





Description.	Breaking stress per square inch.	Elongation in 8 inches.
		Inches.
A.55. $\frac{1}{2}$ inch punched and galvanised .	26·5	·12 cryst.
B. 5. $\frac{1}{2}$,, ,, ,,	30·7	·08 ,,
C. 5. $\frac{1}{2}$,, ,, ,,	28·4	·04 ,,
D. 5. $\frac{3}{8}$,, ,, ,,	28·9	·16
E. 5. $\frac{3}{8}$,, ,, ,,	22·3	·08 cryst.
F. 5. $\frac{3}{8}$,, ,, ,,	29·3	·08
Means . .	27·7	·09
A. 6. $\frac{1}{2}$ inch, punched plain and riveted .	29·4	·16
B. 6. $\frac{1}{2}$,, ,, ,,	30·7	·16
C. 6. $\frac{1}{2}$,, ,, ,,	30·5	·16
D. 6. $\frac{3}{8}$,, ,, ,,	31·4	·16
E. 6. $\frac{3}{8}$,, ,, ,,	32·1	·12
F. 6. $\frac{3}{8}$,, ,, ,,	32·0	·16
Means . .	31·0	·15
A. 7. $\frac{1}{2}$ inch galvanised, punched and riveted .	28·7	·16
B. 7. $\frac{1}{2}$,, ,, ,,	29·7	·12
C. 7. $\frac{1}{2}$,, ,, ,,	23·1	·04 cryst.
D. 7. $\frac{3}{8}$,, ,, ,,	27·5	·12
E. 7. $\frac{3}{8}$,, ,, ,,	29·1	·12
F. 7. $\frac{3}{8}$,, ,, ,,	30·2	·12
Means . .	28·05	·11
A. 8. $\frac{1}{2}$ inch, punched, galvanised and riveted	28·9	·16
B. 8. $\frac{1}{2}$,, ,, ,,	31·5	·16
C. 8. $\frac{1}{2}$,, ,, ,,	28·5	·04 cryst
D. 8. $\frac{3}{8}$,, ,, ,,	27·0	·12
E. 8. $\frac{3}{8}$,, ,, ,,	30·0	·04 cryst.
F. 8. $\frac{3}{8}$,, ,, ,,	29·6	·12
Means . .	29·25	·11
A. 9. $\frac{1}{2}$ inch, hole drilled .	30·6	·28
B. 9. $\frac{1}{2}$,, ,, .	32·0	·24
C. 9. $\frac{1}{2}$,, ,, .	32·1	·20
D. 9. $\frac{3}{8}$,, ,, .	30·9	·24
E. 9. $\frac{3}{8}$,, ,, .	33·2	·24
F. 9. $\frac{3}{8}$,, ,, .	32 0	·24
Means . .	31·8	·24

Comparison of foregoing Tests.

Nos.		Difference in breaking stress per square inch.	Difference in elongation on a length of 8 inches.
1—0.	Effect of galvanizing upon plain steel, 30·4–29·6; 19·6–19·1 %	+ ·8	– ·5 %
3—0.	Effect of punching upon plain steel, 28·0–29·6	– 1·6	
4—1.	Effect of punching upon galvanized steel, 28·5–30·4	– 1·9	
4—0.	Effect of galvanizing and punching upon plain steel, 28·5–29·6	– 1·1	
5—3.	Effect of galvanizing upon punched steel, 27·7–28·0; ·09–·15	– ·3	– ·06 inches
5—0.	Effect of punching and galvanizing upon plain steel, 27·7–29·6	– 1·9	
6—3.	Effect of riveting upon punched steel, 31·0–28·0; ·15–·15	+ 3·0	—
6—0.	Effect of punching and riveting upon plain steel, 31·0–29·6	+ 1·4	
7—4.	Effect of riveting upon galvanized steel punched, 28·05–28·5; ·11–·12	– ·45	– ·01 inches
7—0.	Effect of galvanizing, punching and riveting upon plain steel, 28·05–29·6	– 1·55	
8—5.	Effect of riveting upon punched steel galvanized, 29·25–27·7; ·11–·09	+ 1·55	+ ·02 inches
8—0.	Effect of punching, galvanizing and riveting plain steel, 29·25–29·6	– ·35	
9—0.	Effect of drilling upon plain steel, 31·8–29·6	+ 2·2	
9—3.	Gain in drilling as compared with punching, 31·8–28·0; ·24–·15	+ 3·8	+ ·09 inches

DISCUSSION.

Sir Nathaniel Barnaby: My Lord and Gentlemen, you were good enough to ask me yesterday to provoke a discussion on the first Paper and I succeeded in doing so; I do not know whether I am going to provoke one now by the few remarks I am about to make. I have listened to the Papers that have been read this morning with very great pleasure. They concern, so far as they have gone, the ships of the mercantile marine, and they are on that account most interesting to the great bulk of those who are here. My own association with steel has been mainly with the ships of the Royal Navy. Much has been said in former Papers on that question, and it is pretty well known that we use steel, I may say, exclusively now in the building of ships of war. Our attitude with regard to it is somewhat different from that of the builders of merchant ships, because they have only at this moment reached the point when the prices of steel and iron come together; but we at the Admiralty

were in the position, that for very many years we were paying fancy prices for the iron of which we were building our ships of war. We had to insist upon tests for the material of which the ships of war were built, which tests caused the rejection of very large quantities of material made by the best makers in England, and the consequence of that was that we were paying exceedingly high prices, and that will be understood when I say that we are building the ships, or my successors are building the ships, of the navy of splendid material, such as that which has been described this morning, costing less than half what we were giving for iron, iron giving us immense trouble, at the time when this change took place. Therefore, for us in the navy, the necessity for the change was a very pressing one, a much more pressing one than it has ever been upon the mercantile marine, and it is only now, as the meeting perceives, that in the mercantile marine the real merits of the two materials come before them, to judge as to which is the better for their use. Speaking of the advantages of the material, as a material, so far as our experience has gone, I may cite a case which was spoken of to me privately while the Paper was being read, namely the case of the "Leander," a ship which was built for the Government upon the Clyde, by the firm of Messrs. Napier. She was built with somewhat slight scantlings and thin plates; with a double bottom through a considerable portion of her length. She had the misfortune at the very outset of her career to be rubbed very hard indeed against a rock which is known as the Hornet rock. I went down to see the vessel in dock, and I discovered that the plating along a very large portion of the length had been forced in between the frames, and remained in, the indentation between the frames being about 1 ft. I discovered to my very great gratification that the material was so good, and the work which had been put into it was so good, that none of the riveting either of the edges or of the butts had given way. It was all perfectly sound. A quantity of water came into the ship, but it came in in this way. There were stiffening frames across the plates—the ordinary transverse frames of the ship. They were rather far apart, but they were there, and there were rivets through those frames, and the centres of the plates which they supported. Where the rub had come hard upon those places, the frames had bent in, and in bending in had sheared the rivets which connected them with the plates, and the points of the rivets fell out. The leaks, therefore, were caused simply by the presence of stiffening frames which were not necessary for the strength of the ship, and were only put there for the purpose of stiffening the bottom plating, and it becomes a curious question whether the ship would not have been better if she had not had the stiffening frames at all, but had depended entirely on bulk heads and longitudinals. It is extremely likely in that case she might have got off without any leakage. Our experience fully bears out what has been said this morning in the very able Papers we have listened to from Mr. Martell and Mr. Ward. I should like to say with regard to the action of Lloyds' surveyors, that I think they are wise in being very careful still about the material. I say here before makers of steel and to the shipbuilders, *do not trust the makers.* Trust your own eyes: and be very careful about your material. If any gentlemen here object to what I say, I cannot help that. My experience justifies me in saying that I hope we shall never get into the bad condition with regard to steel that we were once in with regard to iron. Then, when somebody put his brand upon it, it was understood that it must be very good material and we might confidently use it. We were running the risk in that way of getting very bad material into our ships and we did get it. If ever we trust the makers, although they are English makers, and the best English makers, when they put their brand on material and

tell us that is first-rate stuff, if we use it without tests we shall soon be in a dreadful hole. There is but one thing for it. Have your inspectors upon the premises: have there the best men you can get and never cease your own watchfulness. It is a splendid material, a magnificent material, but bad steel can be made and will be made if we do not guard ourselves against it. While, therefore, one is very glad to hear of the success Mr. Denny has had in treating the material in the way he has told us, one cannot help being a little timid lest there should be so much confidence put in the material that we should go wrong, and find ourselves by-and-bye disappointed, whereas if we go carefully there will, I believe, be no reason whatever for disappointment; but we shall have, for many years to come, a material that will answer to all the expectations of the shipbuilders and marine engineers.

Mr. A. C. Kirk: My Lord, you have been so good as to ask me to say something on those Papers, but you must excuse me if what I say is very brief indeed. I see many around me who are very much more capable of speaking of what was dealt with in this Paper, namely, steel for shipbuilding, and I hope they will give us the benefit of their experience. It is certainly very encouraging at this time, after some eight or nine years' experience of steel in shipbuilding, to have our good friend Mr. Martell, with his quite exceptional opportunities of studying the effect of wear and tear upon steel ships, bearing the strong testimony he has done in their favour, to the excellency of the material, and to the fact that if it is at all properly treated it is not subject to more wear and tear than iron. I am also very pleased to find that Mr. Ward's experience quite bears out our own, and the experience, I suppose, of every shipbuilder here, that steel does not require to be nursed or treated in any way other than iron would be treated. (Hear, hear.) The late Mr. George Wilson (of Messrs. Cammells), whom we have recently lost, told a story on this very subject. The first maker of cast steel table knives at Sheffield when he got the steel in, gave his men very special instructions about welding on the wrought iron tangs or ends to which the handles are fixed. The men did not succeed with the welding; and they came to him and told him that they must go back to old sheer steel. So he had the cast steel taken back, and packed and put up, invoiced, and so on, just as the old sheer steel used to be, and told the men nothing about it. They then welded the tangs on to the ends of the knives and there was never any more trouble. Now that is exactly in accordance with my own experience. It does not do to give a man a special lecture on the subject of handling steel, of being very careful of doing this and not doing that. It does not require it, and if it did require it I do not think it would be a trustworthy material to use. With regard to Mr. Martell's Paper, I think there is very little indeed to criticise. It is a Paper we are all very thankful to receive. There is one point, however, upon which I should like to say a word. Mr. Martell relates the effect of a fire upon the sides of a ship, and he draws the conclusion from that that local heating is a thing to be avoided. Well, I confess, I do not see any connection whatever between the two. As a fact, no doubt, the fire destroyed the side of the ship and rendered the steel brittle; but the fire was a fire affecting a large portion of the riveted structure all over; and I have no doubt whatever, for it seems so on the face of it, that the plate remained for a good long period at a red heat and exposed to the atmosphere. Some time ago in this room I related an experiment I made of leaving a piece of plate red hot in a boiler work furnace for twelve hours and allowing the air to pass over it. The result of that was to render the plate quite brittle, and another plate kept as long at a white heat in a forge furnace where no oxygen

could get at it, was rendered rather softer than it was before, and, in fact, rather better. Possibly this is simply a case of a plate being kept for a considerable period red hot, and exposed to the air, becoming in that way brittle, what a blacksmith would call burnt. Neither would I be quite prepared to endorse all that Mr. Martell has said about the impropriety of straightening a plate which has been once bent. I confess I have no experience that would bear that out. It depends; every case must of course be judged on its own merits. A great deal depends upon the extent to which the plates are bent, but I do not think I have ever seen anything to show that steel which has been bent to an ordinary degree, say to a 3 in. or 4 in. radius, to a right angle or something of that kind, and straightened red hot, is at all injured. There is one thing too which we must not forget when we discuss the behaviour of steel, that we ask steel to do a great many things that we never asked iron to do, and we never dared ask iron to do. With steel, we go round a heavy flange eleven inches deep, as I have done often enough, knocking it down with a steam hammer. I have not had a case of failure. If it had been a piece of iron plate I dare not have done such a thing at all, the first blow would have broken it right through. Again, to flange a steel plate for a furnace mouth we push a die right through at one heat which we dare not do in iron without great risk. While I am upon this subject I must mention that this is a peculiarly strong case of local heating. We cut a hole of say three feet diameter in a boiler plate, and we heat about eight inches red hot round the circumference of the hole in the middle of the plate; we push a die through, and we form a flange, or we heat the edge of a plate in lengths of three feet or four feet at a time and knock the flange down with no evil result in either case. The plate is afterwards heated red hot in a furnace, but you cannot allow it simply to lie and cool. It would twist out of shape. It must in the process of cooling while the heat is falling to a blue heat, and perhaps lower, be kept in form forcibly under perhaps considerable strain, or get some readjustment by hammers to restore the proper form as it departs from it. The same is true of nearly every thing you form at a red heat, and thus a certain amount of working or straining at a blue heat is unavoidable. What I have said I think is as much as I am entitled to say on the subject.

The Hon. George Duncan: My Lord and Gentlemen, I have come up to this tribune because nobody will say a word on behalf of the boiler makers in England. I am very sorry to say I cannot agree with Mr. Ward. I cannot agree with him that a steel plate which has been locally heated, and not subsequently annealed, is not in a condition to have a very great strain upon it. I have constantly found that when the platers have in spite of positive instructions attempted to use plates in this condition they have almost invariably cracked. We have now put up machines to taper off the corners of steel plates, cold where the joints come, for the express purpose of avoiding the necessity of re-heating the large flanged plates. The same rule applies to the outside shell plates of the boilers. We nibble the joints out cold where the butt strips are fitted and tucked under the outer shell. We have been making experiments latterly with the flange plates, to ascertain what effect flanging has on steel plates; I find in every experiment hitherto made that for several inches from the flange there is a distinct loss of tensile strength, and a distinct loss of elongation. I should say it amounts at the bend of the flange where it is most severe to about ten per cent. I have heard that the Iron and Steel Institute have prosecuted this enquiry further; perhaps some gentleman will deal with it this afternoon, as it bears very strongly upon the question of local heating and binding. I think that is really all I

can say, except that we are following the Admiralty rules, and I do not think the Admiralty Inspectors would allow us to put a plate into the boilers which had been flanged, or otherwise heated locally before it has been annealed.

Mr. H. H. West: My Lord and Gentlemen, I would very much rather have waited patiently until a later time in the discussion before speaking, but as your lordship has called on me twice, I could not but answer to the call. I am sure we must all rejoice, and rejoice greatly, to hear Mr. Martell's assertion of his unshaken, I was going to say unbounded, faith in steel, which is as great now as ever it was, or greater; and I am glad to hear him say that he has this unshaken faith, because I think it will assist him in removing from the steel makers all unnecessary restrictions upon their manufacture. At the same time, I would venture, if he will allow me, to caution him against removing any restrictions that are really necessary. I quite agree with Sir Nathaniel Barnaby that there is an amount of care necessary in the production of a material which shall be suitable for shipbuilding; and when we bear in mind that Lloyds', as did also the *Underwriters' Registry*, when it existed, allow important reductions in the scantlings of steel used in shipbuilding, on account of its superior quality, I think all will agree that every effort should be made to secure that its superior quality is maintained, otherwise the reductions would be given without any sufficient reason. And I would be equally stringent with regard to iron. I have expressed in this room very strongly my opinion on the quality of iron used for shipbuilding; and a rather remarkable comment on the discussion that arose upon my Paper on the subject was the fact that within a few months Lloyds' issued a circular stating that if the quality of material did not improve some very serious consequences would ensue. I do not know whether it is worth while to take a little credit in passing for one point that Mr. Martell lays a good deal of stress upon, and that is the testing of the material at the steel-maker's works. I believe the *Underwriters' Registry* were the first to do that, and I believe it was very largely due to my own exertions in the matter. The point as to corrosion, which Mr. Martell refers to here, I think with him, has been fairly met by care in preparing the steel for receiving its first coats of paint, and in keeping it well painted thereafter. I know that in one case the corrosion of a large steel sailing vessel was so marked, that I had an impression taken of the pits on the vessel's side as a permanent record, and they were of a very marked character indeed. Notwithstanding that very serious corrosion, next voyage that vessel was apparently no worse, not a fraction worse; and after two or three voyages and two or three coats of paint in the graving dock, there seemed to be no further trouble whatever. I think there is no need to have any concern or any anxiety on that score, if owners, as they always should, will look after their own interests. As to the case that Mr. Martell quotes of a large Atlantic steamer being damaged by fire, I would just like to ask him a question as to whether water was played on the vessel's side from a hose while it was hot? He makes a remark in reference to contraction as the result of "sudden cooling," which suggests to my mind that something of the kind had been done. He will very likely be kind enough to answer that question in his reply. I entirely agree with Mr. Martell in the view that he expresses, that, in the event of damage, it is important that the surveyor should look at it from the point of view of his own responsibility for the efficient performance of the vessel after the repairs have been completed. But I venture to say that from that point of view the surveyor has little or no concern with the owner's

interest as a commercial question. He may be perfectly satisfied that the vessel is as efficient as she was before, without necessarily falling in with the view that she is, as Mr. Martell puts it here, a constructive total loss. The question of insurance has been imported into the Paper, but whether it is a question that should be discussed by this Institution I hardly know, but as it has been imported into it, I would say that the owner should be entitled to get, not the same ship, because that is impossible, but he should be entitled to get as useful a ship as the ship was before the accident, and that if the vessel can be restored at a reasonable cost to as useful and serviceable a condition, either for use or for sale, as she was in before the accident, the owner's interest is fully and properly protected. Mr. Martell seems to suggest, I think, that if plates are supposed to be strained—I do not know that I can say more than supposed to be strained—it is desirable to re-rivet them, and not only that it is desirable, but that they should be re-riveted. Now, I confess that, for my own part, I would be content with a liberal dealing with the slackened and started riveting, removing all those that were distinctly damaged and started, and also those that were in the immediate neighbourhood of the started and slackened rivets; but why riveting that is apparently sound, and, as far as any human means of testing can discover, is in all respects satisfactory should be cut out, I fail to see. Mr. Martell seems also to raise an objection to judging the condition of a butt by testing it with a thin knife. Well, I quite agree with him, that though a butt will not admit the thinnest knife, it does not by any means follow that it is perfectly sound. There are numbers of butts that will not admit the thinnest knife that have undoubtedly been strained; but when a butt is not only in such a condition that it will not admit the thinnest knife, but that it will not admit anything, and is absolutely close, then it seems to me that you cannot go any further. What more do you want? You cannot make it better by any treatment that you can put upon it. I believe that Mr. Ward speaks of the injury to plates from cutting out the rivets in the way of damages, and so forth; and he says that, whether it be from the annealing effect of a hot rivet or whatever else, or that the injury is purely imaginary, he cannot say; but I confess myself, I think it is an injury which is purely imaginary. I am glad to see that so practical and experienced a man as Mr. Ward thinks that steel should be of such a quality as to stand any amount of rough usage in the ship-yard that it would ordinarily get. That is and always has been my opinion, and unless we can get a quality of material that will bear that usage, I think it is infinitely better that we should not have it for merchant ships, whatever is necessary for the navy. All the testing we have, I believe, is addressed, and ought to be addressed, to finding a material that will stand that abuse, and unless we get it, as Mr. Ward says, I think we are better without it.

The PRESIDENT: Gentlemen, I have one suggestion to make to you. It does not appear to me that the question of insurance need be in any sense whatever imported into a purely technical discussion as to the merits or the demerits of steel for ship construction. It is foreign to the subject we have in hand, and it may raise questions which we would much rather not have raised. Therefore if you approve of the suggestion, I will ask subsequent speakers not to touch upon the question of insurance, but to deal simply with the technical question before the meeting as to the material.

Sir JOHN HAY: Lord Ravensworth and Gentlemen, it is a more formidable trial than I had anticipated, this appearing in the tribune to ask a question, for after all it is merely as a questioner

that I will venture to intrude upon the meeting. It so happened that a great many years ago I was associated with the late Sir William Fairbairn and other eminent persons in an enquiry with reference to the iron plate committee for the navy; and at that time we would have given a great deal to have had the information which Mr. Martell and Mr. Ward have put before us to-day. But there are one or two points which have not been touched upon in the Paper, and yet which I think, my Lord, might be fairly discussed (and I dare say Mr. Martell himself or Mr. Ward will kindly answer as to them when they reply), and which have occurred to me as being worth considering under these conditions. Mr. Martell made some observations with regard to a certain limit of thickness, but he does not specify what the limit of thickness is, but I dare say he will mention what that is when he comes to answer the questions which have been put. But the point that I want to put before Mr. Martell and Mr. Ward is the question as to the condition of the mild steel after it has been brought into use for us sailors. I suppose mild steel means, as we all know, iron which is quite free from phosphorus or sulphur, slightly more or less, carbonised. I do not know whether Mr. Anderson is here to-day; he was here yesterday and could speak upon this question which I am now going to ask, more fully. Certain experiments which were conducted by Sir William Fairbairn shewed that the result of constant vibration upon the best qualities of iron and steel was to change its character very rapidly; at least not very rapidly, but to change its character absolutely from a fibrous to a granulated condition, and in changing the material from the fibrous to the granulated condition, of course you lose the tenacity, which is the quality most desirable in mild steel used for the purpose of shipbuilding. Mr. Martell points out in his Paper that the French Government, in applying their steel for the purpose of shipbuilding, laid great stress upon the fact that it should not be hammered, but that it should be bent "under gradual pressure," I think, is the expression, and that is because of the fact, as I fancy we all know, that the tilt hammer produces granulated iron and granulated steel, and the rolled steel, especially cold rolled steel, is especially fibrous. I believe I am right in that suggestion, and from the moment the shipbuilder or the sailor gets hold of the ship, the mild steel is being subjected to incessant blows. Whether it be owing to the action of the riveting or whether it be owing to the action of the screw propeller in propelling the ship, or to the hammering the ship gets in a gale of wind, putting aside altogether the question of the concussion caused by the firing of guns on board a man-of-war, or resulting from rough usage in collision, and so forth, the result is, that a change is effected in the whole material of the mild steel from the time the mild steel comes from the maker and the time when the ship comes back from her cruise, and it is changed gradually from the fibrous and tenacious character to the granulated and more fragile character; at least, that is my belief, and I should like to hear an opinion from Mr. Martell or Mr. Ward upon that, and also from other gentlemen who perhaps may speak upon the subject. There is certain corroborative evidence of that, because, as we all know, the magnetic character of a steel bar is increased by continuous vibration, continuous agitation, or continuous blows, and the late Sir Frederick Evans, whose loss to science and to the navy we must deplore, and who was the best authority on these matters, pointed out that the necessity of constantly swinging the ships in order to ascertain the error of the compass, was due to the fact of the change of the electrical condition of the iron ships, due to the hammering they received at sea, and to the fact that under varying conditions different parts of the ship became more or less magnetised. As for instance, if a ship proceeds against a strong gale to N.N.W. she is equally magnetised on all sides, whereas if she comes down

the trade she gets a hammering on one side or the other going to the south-east or the north-west, and that particular side of the ship is more or less increased in magnetism. It occurred to me that it might almost be possible, by means of the electrometer and without taking the ship to pieces, or submitting her to an expensive examination, to discover whether this change from the fibrous to the granulated character might not be ascertained by some such an experiment. I throw it out for enquiry, and without at all attempting or pretending to dogmatise upon the matter, but feeling as I do, that a steel ship must more or less be changed from its fibrous character to the granulated character by the ordinary action of the vibration of the engines and the ordinary action of the sea, I think it is almost worth while to ascertain whether some such cheap form of examination might not be available. There is one other point upon which I should like to have some information from the lecturers, and that is with reference to the effect of galvanising. We are informed that the galvanising of iron weakens it. Has it the same effect upon steel? Because, of course, it is a great protection against corrosion, and I think it would be interesting to know whether steel is equally liable to be damaged by the application of the very cheap and useful process which prevents the corrosion, which is so very dangerous and disadvantageous to our ships. I really think I need not detain the meeting longer. These are points which occurred to me as being worthy of being answered. I hope they are germane to the subject of the two Papers, and within the scope of the interesting enquiry which has been originated by the able Papers of Mr. Martell and Mr. Ward.

Mr. P. Gilchrist: My Lord and Gentlemen, there are two points, and only two, which I would like to raise upon these very interesting Papers which we have heard, and the first is—Is 27 tons strain the best bottom limit for steel for shipbuilding? I think it is the experience of everybody that the softer steel is the more abuse it will stand. To put it in other words, a good amount of abuse will hurt very little a steel of 24 tons strain, but it will make steel of 27 tons or 28 tons strain perfectly brittle. That being so it is worth while sacrificing that power of standing abuse for the sake of these few extra tons strain, especially as there will not be three tons difference between the two elastic limits. It is quite an engineering question, and one which the builders of ships may well consider (the more so as the steel used for shipbuilding is replacing a fairly bad material, to have 22 tons with the grain and 18 tons against, which is giving it more than it deserves). Whether as it displaces such a material and a material with very little elongation, and a very little contraction of area, such a steel of 24 tons strain which has fully 30 per cent. elongation, generally 35, and over 60 per cent. contraction of area, would not be a fair *quid pro quo*. Mr. Martell has raised another point which I think I may perhaps refer to, and that is whether basic steel is suitable for shipbuilding or not. I do not know whether I have your permission to refer to that. Perhaps it may be interesting, as the process has been used a great deal more abroad than here, to mention what has been done abroad as well as here. Two or three years ago—I forget exactly when—there were those failures which Mr. Martell refers to in Germany. I believe those failures were due really to the material not having had sufficient work upon it, as small ingots were used. But with reference to England, I think we stand in rather a better position than appears on the face of it. There are two works in England, and practically only two who have prepared material to pass Lloyd's survey. One of those works supplied it on a small scale, but I believe I am correctly informed when I state that Lloyd's surveyors had no more trouble with the

material that was presented to them from that one works, than they had with the material that was presented to them from the ordinary acid-Siemen's Works. Mr. Martell will correct me if I am wrong, but I believe I am right as to that. With reference to the other Works it has been very difficult to get information; but I am quite certain of this, that *all* the material, which was sent back from the other Works, was not all basic. I know for a fact that some of it was acid-Siemens, and some of it was acid-Bessemer. But Mr. Martell has better information than I have, and if he has sufficient information to say I am wrong, well and good. But I am certainly positively informed that the material sent back—I have my information from the users of it—was not all basic, therefore if you condemn basic on that you should condemn the other processes. With regard to the uses of basic steel for shipbuilding abroad, in Austria I am glad to say it has been used, and also in France; in the latter country one of the largest works there wrote me not long since, to say that they had been using it for some time for shipbuilding, sending it out side by side with their acid steel, that the quantity of it was continually being increased, and as to results, they heard no complaints whatever. The custom over there is a little different from what it is here. They have as we have tests, but they have sufficient confidence in those tests to say, If it stands those tests we pass it. On those terms it has gone in, and it has never been found out that it is not as good as the acid steel.

Mr. James Riley: My Lord and Gentlemen, I am sure you will understand that it has been a matter of peculiar pleasure to me to be present this morning and listen to the remarks which have been made by our friends on these two Papers. Contrasting as I do the state of feeling which existed when ten years ago this day, I think, I stood here and read the Paper to which reference has been made, with the progress which has been made as shown in the Papers read by Mr. Martell and Mr. Ward, I think I may say it has been wider, greater, and more satisfactory than the most sanguine friends of mild steel of that day could have ventured to imagine would be the case. There are several matters in the Paper of Mr. Martell which you may be sure I have listened to with pleasure, and amongst them I may mention the affirmation and re-affirmation, if that were necessary, this morning, of his fullest confidence in the sterling qualities of mild steel, and the statement of his experience in the use of it during the last eight years; and in the case of Mr. Ward the marvellously few failures he has had to report. All these things seem to me to strengthen the appeal which Mr. Ward made. Why should it be under discussion at all? Why not accept the matter as finally settled? Here is a material which we had presented before us ten years ago, which we have been trying and testing and talking about in every possible way for the last ten years. Why cannot you do as you did with the inferior material, iron, and accept it and give it your fullest confidence? I was glad to observe also that in neither of the Papers, nor so far as I have heard in the discussion, have there been used those ominous words so dreadful to makers of steel, "mysterious qualities." For once I have been at a meeting when they have not been used, and I have not had a cold sweat thrown upon me. It appears to me that the mystery has vanished, not because there were any mysteries to vanish really, but because our knowledge has increased, and if we had been frank, and honest, and clear, we should have seen, not that there was a mystery about the material, but rather that our ignorance was the cause of the trouble. We have had reference this morning to local heating. I do not know, although I have this great confidence in mild steel which other people have, that I dare

venture to go as far as our friend Mr. Ward has, and yet I am bound to say that for the last six or nine months at intervals, I have been trying to fracture plates by local heating, and in no single instance have I succeeded, and yet it does not seem to be correct; theoretically I ought to have broken the plates, but I have failed in every case, and these cases have been very numerous. Secondly as to the matter of working at a blue heat. This is undoubtedly in my mind an extremely dangerous thing, and ought to be avoided. In every case our conclusion has been—by ours, I mean the steel manufacturers—in every case or nearly every case that has come before us of these mysterious failures that they have been due to the working of the plates at the blue heat, and under certain conditions. If there are sharp corners where you punch out the corners in preparing to flange a plate, if that punching is done with a small punch, a sharp corner is punched in, and unless you are extremely careful in working locally, and if you do not avoid working at a blue heat, you will have a crack set up at that particular point. It is almost certain you will accomplish it. But there is another danger, that is to say the one of overheating, which has not been referred to so much as perhaps it might be. If you overheat steel—say you are annealing and overheat it to what we consider a dangerous extent, when it is coming to an orange temperature, or rather an orange colour, you are doing a serious mischief to that plate unless it be rolled again or worked in some particular manner. An extremely dangerous thing, more so perhaps than any other thing which has been referred to, is overheating, without work being put on at that stage. These things have been well referred to in a discussion at the Civil Engineers which has lately taken place, and we have verified them from time to time. You not only reduce considerably the tensile strength of the material, but you take away almost entirely the ductility of the material if you overheat. Another point which I may refer to is the effect of annealing. I do not think enough has been made of that, and particularly I may perhaps mention a case which seems to bear on the point under discussion this morning, as to the taking out of damaged plates. The effect of annealing is to restore plates which have been deformed and injured to the primary condition of the material. Take a test. I believe this has been done in other places, but we have done it at our own Works and it is an extremely interesting one. You may take a piece of steel, say of 30 tons strength, and apply a strain of 26 tons to it, and you will get an elongation perhaps of six or seven per cent. Stop there; anneal the piece; proceed again to apply a strain of 26 tons and you will get again six and a half per cent. of extension, and it seems as if you could repeat that any number of times that you please. I have had it done up to six or eight times without any deleterious result to the plate. The effect of annealing has been to give back the properties which the steel possessed in the first instance, its strength and its ductility. I do not know that I should refer to the question of rivets to which Mr. Ward has alluded, but it is rather startling to find that a considerable number of rivets which have been put into a structure, whatever it may be, are found when knocked out to be fractured, but as a steel-maker perhaps it is a comfort to know that the same results are found where iron is used. The other day we had to take to pieces to repair and renew the boiler of a heavy crane of ours, and on examination I found that of the iron rivets which were knocked out, twenty-five per cent. were fractured more or less, some of them quite through, and as Mr. Ward stated at the counter sink end, close to the counter sink head. In the same way I may mention that we have a number of very large and powerful locomotive boilers, and the stays are rather short between the fire box and the shell. After being at work for some months—it is some five years ago

now that we set them to work—we found these stays failing. They were of steel, our own steel, and this caused me considerable anxiety. The boiler insurance company came down to examine and report thereon. Mr. MacDougall pressed me very hard to take out the steel and put in iron. You may understand that it was with considerable reluctance that I agreed to that, but I did agree to it, and the steel stays were every one taken out and replaced by Lowmoor iron, the very best we could get. Singularly enough exactly the same effect was produced on the Lowmoor iron stays. After working for nine months they appeared as if they were utterly fatigued, and broke, and we replaced the Lowmoor iron stays with steel ones made from our own steel, and the stays have been of steel ever since. But, Gentlemen, I think it may be permissible for me to say that there is steel and steel; you have ignored that, to a large extent, I believe in many of these discussions. Why should there not be some credit given where extreme care has been taken, and where if you please greater experience has been brought to bear? Why should there not be the credit given? Why should you place all on the same platform? and say that all the steel that is made, no matter by whom or by what process, shall be equal in character, and expect the same results from it. You do not act thus in any other business. I can assure you from my own knowledge of what takes place in our own works that there is steel and steel. Whether we admit it or not there is sometimes imperfect steel made which has to be broken up and remelted. The point is to take care and keep it at home. It would be a delicate matter for me to refer to the points raised by the last speaker. I do not wish to say more of the steel made by the basic process than this one thing. It has long been demonstrated, and we have found the same thing, that steel made by the Bessemer process, the acid steel, is not equal in uniformity and reliability to the steel made by the open-hearth acid process. As I stated some months ago in the hearing of my friends, I believe that the outcome of all the experiments will be that basic steel will be made absolutely reliable and uniform in character; but it will not be by the Bessemer process but by the open-hearth process. I do not know, my Lord, that I need detain the meeting with any further observations.

Mr. W. Parker: May I ask Mr. Riley before he sits down if he will kindly give us the position of the stays to which he refers in the fire box. Where were they situated?

Mr. Riley: All round the fire box, just as in an ordinary locomotive fire box, all round and over the crown. They were all round the fire box.

A Member: Where did they give way?

Mr. Riley: Close to the outer shell. Some of them broke right through. In nearly every case the failure was close to the shell, as if some little freedom had been given because of the fire box being thinner and more flexible, and yielding a little more to the stay than the outer shell.

A Member: Were the dimensions as they stood ultimately different?

Mr. Riley: No; they all remained the same I believe.

A Member: Of similar quality?

Mr. Riley; I believe so; but may I just add I am afraid that shows a misunderstanding. I

have in making my account taken into consideration this fact, that every nine months at the longest I have got to replace these stays. They will all fail, I do not say the steel does not fail, but I say it stands as well as Lowmoor iron.

Mr. W. Parker: My Lord and Gentlemen, in making a few remarks upon these two Papers I would say in the first place that I think everyone must feel impressed with the plain and able manner in which Mr. Martell has set forth his views. I think his Paper must remove any impression which may hitherto have existed in the minds of shipowners or others as to the unreliability of steel for shipbuilding purposes. There is nothing more I wish to say on that Paper. But in reference to Mr. Ward's Paper I will venture to trouble you with a few observations. Taking this Paper throughout I must say I am somewhat astonished to find that we are asked again to discuss what we considered here some eight or nine years ago, and which many if not all of us believed to have been finally settled, namely, the question whether in the manipulation of steel for shipbuilding, boilermaking, or any other engineering purpose, the material requires to be treated differently from iron. Mr. Ward answers this question with a most emphatic negative. With Mr. Ward's view I must at once say I entirely disagree, as I think the great majority of engineers who have had experience of the use of steel will do. It may be within the recollection of many here that in 1878 I read a Paper on the use of steel for marine boilermaking purposes, in which I endeavoured to point out the effect of punching, riming and annealing iron and steel plates. There was at that time a pretty general belief that steel did not lose anything from punching; and my then colleague, Mr. John, and I were instructed by the Committee of Lloyd's Register to investigate the subject. I see Mr. John sitting here, and he will doubtless be able to corroborate me in what I am about to say. Well, he and I visited nearly all the steel works in the kingdom, and chose plates from various works, plates made both by the Siemens-Martin and by the Bessemer processes, as well as corresponding plates in iron. The specimens were prepared of such dimensions as would truly represent a punched seam. The thinnest specimens were a $\frac{1}{4}$ of an inch thick, and they advanced in thickness by $\frac{1}{8}$ of an inch up to 1 inch in thickness. One set of specimens were punched, another set were punched and rimed, and a third set were punched and annealed. They were taken to a powerful testing machine at Saltney, near Chester, and tested to destruction. The results of these experiments proved that steel plates differed considerably from iron under the action of a punch. The amount of loss in all these specimens varied with the thickness of the plates, and I notice that Mr. Ward forms the conclusions he comes to from very thin plates. The plates he refers to in this Paper are I think only $\frac{3}{16}$ or $\frac{5}{16}$ of an inch in thickness.

Mr. Ward: Half an inch.

Mr. Parker: As I said before, the plates we experimented upon varied in thickness from $\frac{1}{4}$ in. to an inch, and the loss of strength in steel plates, due to punching, was so great, that when we reached $\frac{5}{8}$ in. in thickness, quite one-third of the original strength was gone. In other words, the steel plates lost 33 per cent. of their original strength, whilst iron plates of the same thickness and treated in precisely the same manner lost only 12 per cent. By making the plates warm and allowing them to cool, the whole of this lost strength was restored; also by riming out the portion injured round the hole by punching, the strength was again restored. I have some of the specimens in my office, and I shall be

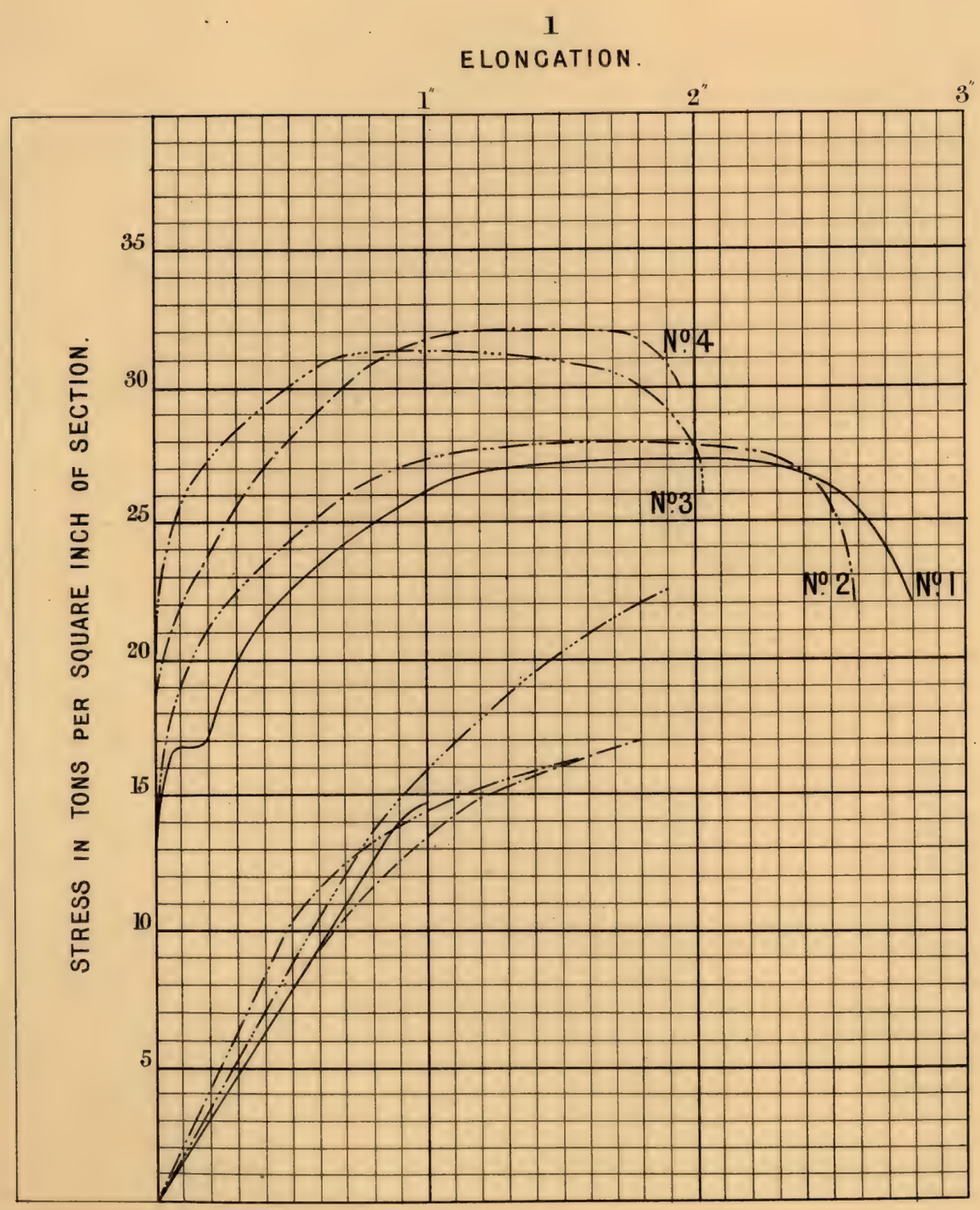
very pleased to show them to Mr. Ward if he will call. I shall also, with your permission, my Lord, send to the Secretary for insertion in the *Transactions* of the Institution, a copy of the Report which Mr. John and I made on this subject to the Committee of Lloyd's Register.* Then again with regard to shearing. I certainly thought it would generally have been admitted, that if you shear a strip from a steel plate, and attempt to bend it in its sheared state, it will undoubtedly break; but if you anneal it, and so set at rest the strains which were produced by shearing, it will bend perfectly. Again, if you plane off the injured edges you will restore it. In the face of these well-known facts, I am at a loss to understand Mr. Ward's reasons for saying that steel is not injured by punching or shearing. Another point upon which I cannot allow Mr. Ward's remarks to pass unnoticed is the question of local heating, in which Mr. Ward sees nothing objectionable. On the contrary, I quite agree with the Hon. George Duncan, who has just spoken, and who I think accurately expressed the opinion of the large engineers in this country, who have had the greatest experience in the use of steel for marine boiler making,—that it is a dangerous practice to locally heat steel plates, unless you anneal them afterwards, because initial strains of a more or less severe nature must be set up in the plate by local heating. I am quite well aware that, although the Committee of Lloyd's Register discourage local heating in boiler making, it is still followed to some extent; but, although that local heating does not produce actual fracture, it is difficult to say how near plates so treated may be to the point of fracture unless annealed. We frequently hear of steel plates cracking or tearing; but we do not always get satisfactory reasons why they give way. Local heating, however, if practised, might account for many curious cases. Reference has been made to the Paper recently read by Mr. Stromeyer at the Institution of Civil Engineers on working steel at a blue heat, and as Mr. Riley says, it has thrown a great deal of light upon the subject. The discussion on that Paper extended over three nights and brought out many valuable facts. At the request of some Members of the Council of that Institution, I undertook to carry the investigation a little further, with a view to determine to what extent steel is injured by being worked not only at a blue heat but under different conditions. In order to do so and to obtain quantitative results, I employed an ordinary tensile testing machine fitted with Wickstead's autographic recorder. Perhaps many of those present do not know what this apparatus is. It is an instrument attached to the testing machine which records or indicates on a card the tenacity, stretch and limit of elasticity of the specimen while being tested, scribing a diagram very much in the same manner as a Richard's indicator does when attached to the cylinder of a steam engine. Figure 1 shows some enlarged diagrams taken by this instrument. The vertical line represents the strain applied and is divided into ordinates, each ordinate representing one ton. The curved line is the line of elongation and is measured in inches and parts of an inch. Diagram No. 1, Figure 1, Plate XVA, is taken from a plain piece of steel. Diagram No. 2 is taken from a piece of the same material which was bent cold to a curvature with a radius of $8\frac{1}{2}$ inches and then straightened. Diagram No. 3 represents a similar piece worked in the same manner as No. 2, but at a blue heat instead of cold. Diagram No. 4 represents a piece that was heated, then quenched in cold water and tested. Diagram No. 5 in Figure 2 represents a similar piece bent in the same manner as No. 2, only instead of being bent once and straightened it was bent four times and straightened, then made red hot and allowed to cool. On reference to these diagrams

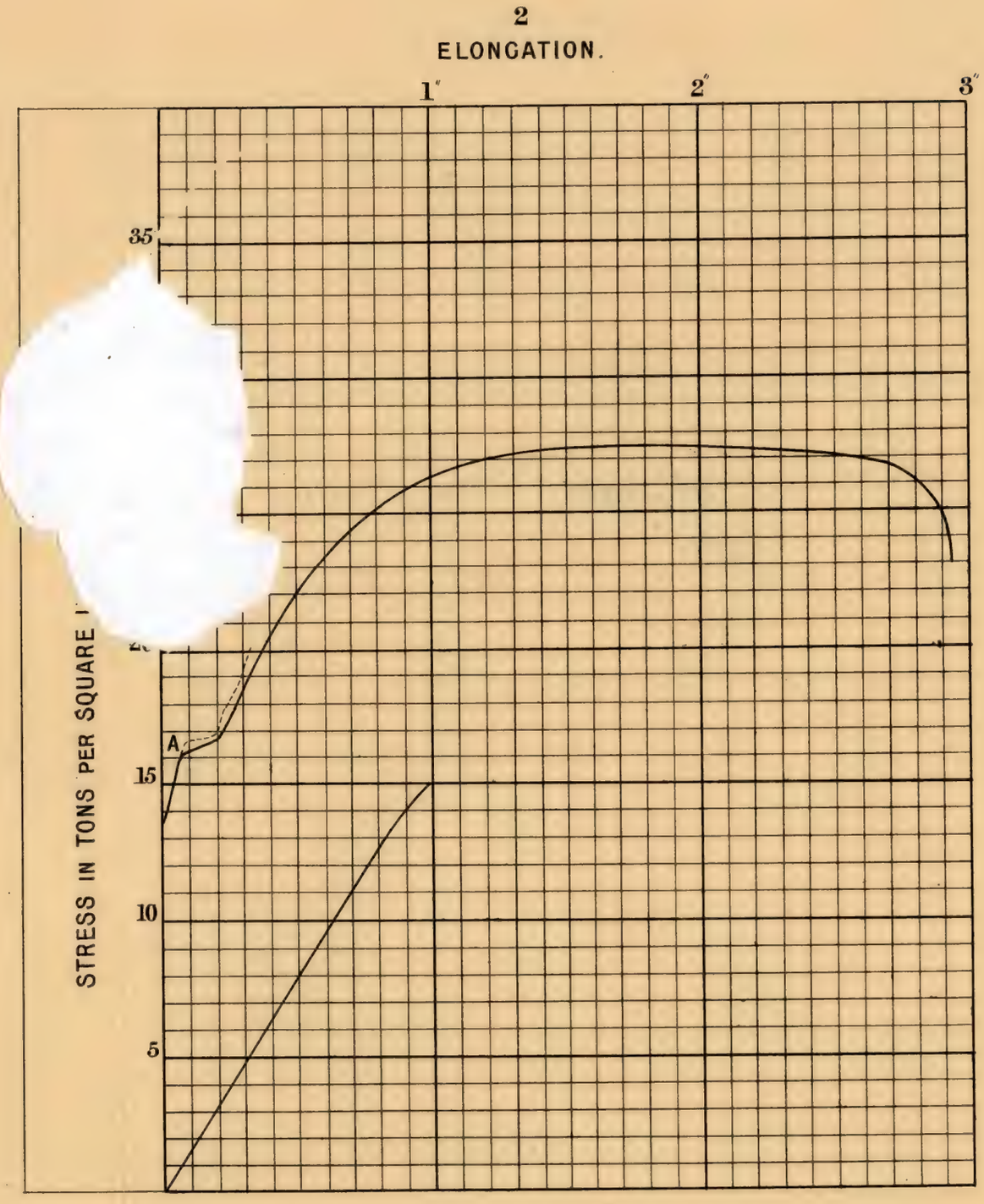
* This Report will be found on page 415.

STRESS IN TONS PER SQUARE INCH OF SECTION.

35
30
25
20
15
10
5

To Illustrate Mr. W. Parker's Remarks on Mr. B. Martell's and Mr. J. Ward's Papers on Steel.





it will be seen very clearly to what extent the material is altered by working and treating it in different ways.

No. of Test.	Dimensions.	Ultimate stress per square inch.	Limit of Elasticity.	Elongation in 10 inches.	Contraction of area per cent.	Remarks.
1	1·5 × ·76	27·2	Tons per sq. in. 14·9	2·78	53·6	Plain.
2	1·5 × ·75	27·75	10·0	2·57	52·6	Worked cold.
3	1·5 × ·75	31·2	13·0	2·07	50·5	Worked blue heat.
4	1·5 × ·76	32·7	9·0	1·76	32·0	Quenched.
5	1·51 × ·76	27·1	15·3	2·92	53·5	Worked cold and then annealed.

The Table gives the ultimate strength at which these specimens broke, the limit of elasticity or break down point, the elongation or stretch in a length of ten inches, and the percentage of reduced sectional area at the point of fracture taken from the diagram. It may be stated that the strips were three-quarter inch thick and two inches wide, and were cut from a Siemens-Martin plate, kindly supplied by Mr. Riley of the Steel Company of Scotland. From these diagrams and tables it will be seen how the physical properties of steel are altered by the material being worked in different ways. Taking No. 1 test, the plain piece, it will be observed that the ultimate strength was 27·2 tons, the limit of elasticity was reached when the strain was 14·9 tons, the elongation in ten inches was 2·78 inches, the contraction of area 53·6. No. 2 test, the piece which had a little work put upon it, shows a slightly increased ultimate tensile strength, namely 27·75 tons, while the limit of elasticity was reached at 10 tons, the contraction of area was 52·6, and the stretch 2·57 inches. In the case of No. 3 test, where the piece was worked at a blue heat, the ultimate strength was increased to 31·2 tons, the limit of elasticity to 13 tons, the contraction of area was 50·5 and the stretch 2·07 inches. No. 5 test piece, which was annealed after having been worked, was entirely restored to the physical conditions in which it was before any work had been applied, as will be seen upon reference to the separate diagram. This restoration is very remarkable; in fact, if the one curve be placed over the other they will be seen to be almost identical. The break down point which disappeared in the specimens that were worked is again perceptible, the ultimate strength and contraction of area are almost brought back to what they were before the work was put upon the material, conclusively proving to my mind that annealing is highly beneficial, indeed necessary, where steel has been severely tortured in working. In carrying out the experiments for the Institution of Civil Engineers, of which

those that I have endeavoured to illustrate form a very small proportion, I have tried to ascertain what changes take place in materials worked under different conditions before the limit of elasticity is reached. As a matter of fact, materials are strained when the first stress is applied, and by the aid of Mr. Stromeyer's very ingenious instrument, which was described here yesterday, I have been able to measure to what extent the properties of the material are altered, and have plotted these results out on curves which are shown on the diagrams, and represent the deflection of the diagram from the perpendicular line magnified one hundred times. These curves are full of interest, and may profitably be referred to again. I think I have nothing further to say, except one word about basic steel. I do not think anybody regrets more than I do the accidents which have taken place in steel plates manufactured by the basic process, and I am quite sure the Committee of Lloyd's Register have the same feeling, but they saw there was no other course open to them than to prohibit the use of basic steel until it could clearly be shewn that the material could be made of a more uniform character. I venture to say, and I may say this especially in the face of the present fact, that there are thousands of tons being made by our foreign competitors, and it does seem disgraceful to us as Englishmen that we cannot do the same thing here. We have Russians making it, we have Germans making it, we have Italians and Austrians making and using it, and if it can be made and used satisfactorily on the Continent, why cannot the same thing be done in England? I had letters from St. Petersburg the other day, upon the accuracy of which I can rely, in which I was told that last year at the Baltic Iron Works alone 800 tons of boilers were made of basic Siemen's steel.

Mr. Parker: These plates were subject to the severe cold in that country, and were worked and flanged in the same manner as ordinary steel plates without a single failure. These facts are well worthy the careful consideration of our steel makers. I think that the great majority of failures in this country have been experienced in basic steel of a comparatively high tenacity, the steel of low strength being relatively much more uniform in character. Why this should be so, and whether it is inevitable in basic steel made either here or abroad, perhaps Mr. Gilchrist or Mr. Riley, who are authorities on the chemistry of steel, will explain. Taking the broad facts of the case as they present themselves to us, however, it does appear as if our continental neighbours had attained to a greater perfection in the manufacture of basic steel than has yet been reached in this country.

Mr. E. Reynolds: My Lord, I have only a very few words to say, and they have been suggested in a great measure by the remarks of the last speaker, on the subject of basic steel, and I only take up your time now because (although I have not seen a public statement of this view before) it seems to have been assumed that, because a certain process will get rid of phosphorus in the manufacture of steel, any stuff which has plenty of phosphorus in it will do to make steel of. I imagine that some of the discrepancies referred to will be got rid of when the same care is taken in the selection of ores to make basic steel from as is taken in the selection of ores to make acid steel, that is to say, when care is taken that you have no serious impurities besides phosphorus to get rid of. There is another very simple matter which I wish to refer to. I think that, in order to place a subject upon a proper scientific basis, little errors or omissions ought to be eliminated, and one has been repeated several times in this room with reference to these very important experiments which have been referred to by

Mr. Parker on the effect of punching on steel. I think I mentioned it to him at the time. The general result of these experiments is that the damage resulting from punching increased in a pretty rapid ratio to the thickness of the plates. Of course, I believe the record of the facts in practice, but I do not in the least believe, as a matter of principle, that such is the case. I believe that, if a plate an inch thick were of the same quality as a plate a quarter of an inch thick, and if it were punched under the same circumstances, the result would be the same, the same circumstances not being that you should take the machine and work at the same magical speed of seventeen strokes a minute, which shipyard workmen seem to like, but that you should take the same or proportional time to remove the same or proportional material, and give the same facility for "flow of solids" in one case as you do in the other case.

Mr. W. Denny: My Lord, I listened with very great pleasure to the two Papers just read, and as references were made in both to the experience I have had with steel and to the Papers I have read about it, I may be permitted to make a few remarks upon them. The Institution has reason to be grateful for these two Papers, inasmuch as many questions in connection with steel have been raised by them, both in a practical and scientific form. But I think we should distinguish in our treatment of these questions the practical experience we have gained in the use of steel from the merely abstract views we hold about it. What my partner, Mr. Ward, has put before you to-day is the experience of our shipyard in working steel under conditions identical with those usual in the treatment of iron. He has, in doing this, given you most frankly every instance of failure we have had with steel made by the Siemens-Martin process. At the time I wrote my Papers, we had been using steel made by the Bessemer, as well as that made by the Siemens-Martin process; but our experience of the Bessemer steel obliged us to limit ourselves to that made by the Siemens-Martin process. I, therefore, ask your careful attention to the fact that the references in Mr. Ward's Paper are only to steel made by this process. Mr. Parker's remarks on the effect of working and heating upon steel are of much interest, but I would like to ask him, before going further, if the elastic limit was determined by the point at which the steel began to stretch, or that at which it failed to return from a stretch which had commenced.

Mr. W. Parker: I tried to define the point where the diagram ceased to be a straight line. I called that the elastic limit. In order to arrive at that a strain was applied, say of five tons, and then taken off; then a strain of 10 tons was applied and taken off; then 11 tons, 12 tons and so on until the point was arrived at where the strain would not go back, and permanent set had taken place. That is the point which I call the elastic limit. By reference to the magnified curve of useful work it will be seen that this point varies in all the specimens that have been differently treated, and is very much lower than would at first be thought. This point in Figure 2 marked A is the supposed elastic limit, but I would prefer to call it the plastic limit.

Mr. Denny: I was only wishing to be informed on this point. But, granting the importance of all Mr. Parker has shown, it is very necessary for you to bear in mind that our experience has been acquired not by dealing with one or two test pieces, but from the work of many years in which the pieces treated are numbered by thousands. You should further bear in mind that, do what you may, you will

never, I believe, be able to eliminate working at a blue heat from the operations of a shipyard Therefore, it should be satisfactory to you to learn from a shipyard's experience honestly given, that the working done in that yard at a blue heat has not been harmful. I am strongly disposed to think that the trouble we used to find in working at a blue heat some years ago arose from the quality of the steel, and that the more perfect the steel the less the risk in working it at a blue heat. The case of the steamer which suffered from fire in New York, to which Mr. Martell has referred, is interesting, but the two features in it, pointed out by Mr. Kirk and Mr. West, will prevent its being applicable as an analogy to ordinary cases of local heating. The fact that these plates were for a considerable time exposed at a red heat to currents of air, and that they were very probably quenched with cold water while still hot, disposes of their usefulness in this discussion. Mr. Parker's table shews you some effects of this quenching in cold water, and it is very probable that this steamer was seriously affected by such treatment. But you will remember such treatment as that is not employed either in a shipyard, or in a boiler shop. In both Papers references have been made to my firm's practice in galvanising light steel river steamers, and from these references one might infer that this practice had been introduced by my firm. This is not the case, it having been originally introduced by the torpedo boat builders, Messrs. Thornycroft and Yarrow. Indeed, before we adopted it we consulted my friend Mr. Yarrow, and had the advantage of his experience. Mr. Martell has made a most interesting reference to the case of a steamer which, when placed in dry dock, was found to have had her butts tight, and yet showed evidences inside of these butts having been leaky at sea. I wish very much that cases of this kind in the future might be so treated as to preserve permanent pictorial records of them for this Institution to examine. I have on more than one occasion pointed out here, that there is no department of naval architecture in which we are so deficient as in the study of the morbid anatomy of vessel structures. If we had as good an atlas of the defects which occur in the skins of ships as we have of the diseases which afflict the skins of human beings, we might make some sure progress in our remedies. But we have still to wait for a systematic and scientific treatment of this subject. It is indeed a very extraordinary thing that among all the Papers which have been contributed to this Institution we have not one dealing with the structural defects, and attempting to classify them properly. For my own part I am inclined to doubt that any butt would leak at sea, which is absolutely tight when thoroughly tested in dock. I would doubt that unless I had the opportunity of examining the cases for myself, or of examining such exact reproductions of them as would enable me to form a sound judgment upon them. Mr. Ward has referred to the extra riveting lately required by Lloyd's in the shell butts of steel steamers. We have a steamer building, of an ordinary sea-going type 315 feet long. Her shell butts are all treble riveted throughout amidships, and have now as required by Lloyd's the outer rows of rivets completed. This is not only a useless but injurious performance. We have vessels at sea of over 400 feet long doing their work well, and exhibiting no defects in their butts, which are treble chain riveted, and with incomplete outer rows. It is, therefore, absurd to require in very much shorter steamers what has been proved to be unnecessary in longer vessels. At one time we did think it advisable to complete the outer row in treble chain riveting, but we gave up the practice because we found that, if a thorough good job were made of the butts, if they were fitted close, and tack riveted to keep them in position, there was no necessity for extra riveting. We have changed our practice in some other similar points, as the result of experience. In our first one or two

steel steamers the fine heels were very liable to leak, and on this account we increased the scantling of their plating, but we found later that by improving the workmanship, and by a slight modification in the section of the heels there was no necessity for these increased scantlings. Indeed, in almost all portions of the work involved in steel shipbuilding we have found the extra thicknesses unnecessary, which we originally thought to be required. I do therefore hope that Lloyd's, who have taken this step, may see their way to retrace it, as we ourselves went back from it several years ago. It is unnecessary, and whatever is unnecessary is an unreasonable and unfair handicapping of steel. I can only conclude, my Lord, by thanking the authors of these two Papers for the excellent matter they have brought before us, and by regretting that the time at my disposal does not permit me to go into many other points I should have liked to have touched upon, but that I know there are many other gentlemen wishing to speak upon them.

Mr. W. H. White: My Lord, I would not have intervened in this discussion (as there are many gentlemen, as Mr. Denny has said, who, no doubt, desire to speak) but for two reasons. One reason is that my friend Sir Nathaniel Barnaby has not alluded to the attitude which has been consistently maintained by the Admiralty in relation to the various modes of manufacturing steel. From the very first the Admiralty has acted on the principle that, while it has nothing to do with details of manufacture, it has a perfect right to choose for any steel plates or bars that may be ordered, the mode of manufacture preferred for that particular steel. I hope I have put the distinction clearly. At first we had only Bessemer and Siemen's steel to deal with: both modes of manufacture were recognised, and steel was ordered from both Bessemer makers, and from those makers who worked by the open-hearth process. That still remains true. Admiralty officers are now about to undertake a series of experiments (preliminary experiments) on steel made by the basic process, and upon the result of those experiments will depend the admission or otherwise of basic steel for use in ships built for the Royal Navy. But no matter whether the modes of manufacture may be many or few, the Admiralty in its specifications reserves the right of selecting the mode of manufacture which it prefers in any particular case. I would next wish to say a few words as to the mode of treatment of steel in the Admiralty service. Like all users of steel, the Admiralty officials have learned more about the material as they have gone on. At first in the "Iris" and "Mercury," every piece of steel in the structure was annealed. After a time that practice was somewhat relaxed, but still annealing was the rule; and I am bound to say that not a few cases have come to my knowledge where annealing has done more harm than good. I am not saying a word against annealing when properly performed: far from that. I endorse everything that has been said in favour of annealing. But as there is (as Mr. Riley said) "steel and steel," so there are modes of annealing and other modes of annealing. There is such a thing as "burning" steel in an annealing furnace. The Admiralty has not in this matter been strictly conservative. As time has gone on, and as experience has enlarged, so the mode of treating steel has been varied; and with the permission of the President, I will put in for record in the transactions a copy of the "instructions for the treatment of mild steel," which I drew up four years and a half ago, and which are still in force. These instructions like all Admiralty instructions are, I hope, treated intelligently by those who have to carry them out. Putting in these instructions will save me any necessity for any further remarks on such matters as local heating,

annealing, &c.: as the Admiralty practice on these matters is explained in the instructions, and will be thus understood by members better than I can describe it now. As regards the questions of "blue heat" and of "local heating," I may, perhaps, be permitted to say one word. They are often treated as distinct, but in practice they are closely related. I have myself seen accidents of this kind repeatedly, where there has been local heating in order to do some work, an unfortunate blow struck at a part where there was blue heat has led to a fracture. Of course, anyone who has had to do with steel working has seen the same thing. But, having seen that, I would like to endorse Mr. Denny's statement, that these cases which we have been discussing, of blue heat accidents from local heating, annealing and so on, scarcely represent the average practice of the shipyard and the boiler shop. Things go on smoothly and without remark until some day there comes an accident, which is certainly the exception and not the rule. Everybody who has had to do with steel, will confirm the favourable opinion which is the common bond of union between the two Papers read here to-day. In the Admiralty service and outside it, we believe in steel much more thoroughly than we thought we should do eleven years ago when the steel for the "Iris" and "Mercury" was undertaken to be made at Landore. As regards riveting, I have said before in this hall, and I will repeat it, that from the very first we only used iron rivets in steel plates, because we could not get steel rivets that we could trust. As soon as we could get steel rivets that we could trust, we used them, and we were glad to have them. As regards corrosion we have ships in the navy that have been afloat for nine or ten years, and the results are satisfactory. There is no practical difference between iron and steel ships in those cases where the manufacturers' scale has been removed by the so-called pickling process. On these points I give the results of enquiries recently made, because my absence from the Admiralty put me out of acquaintance with the facts of Admiralty experience. As regards a remark which fell from Mr. Gilchrist with reference to the desirability or otherwise of having to purchase at some risk an increase in tensile strength, I would like to say: that there are many parts in the structure of a ship where it is advantageous to have the greater rigidity which accompanies higher tensile strength. When one has to deal with light structures such as the thin bottoms of the largest ironclads, which I have heard described by irreverent critics as "tin kettles," but which, nevertheless, have to bear various and severe strains; then, undoubtedly, it is often desirable to have a higher tensile strength than 24 tons, because this higher tensile strength carries with it a large amount of rigidity to bridge over the spaces between the supports. That is a practical matter to which I am sure everyone here would give assent, who has had to do with steel or any other ductile material. I would venture to add, that the Admiralty limits of tensile strength for steel (from 26 to 30 tons per square inch) have been maintained simply as the limits within which the Admiralty inspectors at the works could accept, and pass steel without question or reference. But as Sir Nathaniel Barnaby will well know it has always been our practice, if the test condition of ductility was fulfilled, to raise no objection to the acceptance of steel of higher tensile strength than 30 tons per square inch. I think the time must come when we shall work more towards the conditions of higher instead of lower tensile strength, especially in very light vessels, approaching more closely to the conditions which held good in the earlier steel ships, where the steel used was of a very high tensile strength. Those, my Lord, are the main points which I wished to mention, and with your permission I will now hand in these instructions for record.

Instructions for Treatment of Mild Steel.

1. All plates or bars which can be bent *cold*, are to be so treated; and if the whole length cannot be bent cold, heating is to be had recourse to over as little length as possible.

2. In cases where plates or bars have to be heated, the greatest care should be taken to prevent any work being done upon the material after it has fallen to the dangerous limit of temperature known as a "blue heat"—say from 600° to 400° Fahrenheit. Should this limit be reached during working, the plates or bars should be re-heated.

3. Where plates or bars have been heated throughout for bending, flanging, &c., and the work has been completed at one heat, subsequent annealing is unnecessary.

4. Where simple forge-work has been done, such as the formation of joggles, corners, and easy curves or bends, on portions of plates or bars, and the material has not been much distressed, subsequent annealing is unnecessary.

5. Plates or bars which have had a large amount of work put upon them while hot, and have had to be re-heated, should be subsequently annealed. It is preferred that this annealing should be done simultaneously over the whole of each plate or bar when this can be done conveniently. If it is inconvenient to perform the operation of annealing at one time for the whole of a plate or bar, portions may be annealed separately, proper care being taken to prevent an abrupt termination of the line of heat. If the severe working has been limited to a comparatively small part of a plate or bar, annealing may be limited to the parts which have been heated, the same care being taken to prevent an abrupt termination of the line of heat.

6. If desired, exceptionally long or quickly curved bars, such as frames, may be formed of shorter pieces, with the butts suitably shifted and strapped.

7. In cases where any bar or plate shows signs of failure or fracture in working, the details of the cases should be forwarded to the Admiralty, in order that instructions may be given as to the disposal of the bar or plate.

8. It is not necessary to anneal plates or bars after punching as a means of making good damage done in punching. For plating which forms an important feature in the general structural strength, such as the outer bottom plating, deck plating, deck stringers, &c., all butt straps should have the holes drilled or be annealed after the holes are punched. In outer bottom plating, the holes which are to be countersunk, should be punched about $\frac{1}{8}$-in. less in diameter than the rivets which are used, the enlargement of the holes being made in the countersinking, which should in all cases be carried through the whole thickness of the plates.

9. Snap riveting is only to be employed for the internal work on transverse bulkheads, floors, framing, and other subordinate parts of the structure, but on stringers, deck-plating and other parts subjected to considerable tensile strain, countersunk riveting is to be used, and the holes treated similarly to those in the outside plating.

10. It is important that the whole surface of the bottom plating should be thoroughly cleared of the scale formed in manufacture before any paint or composition is put upon it. Detailed instructions as to the methods of removing the scale are given on N.S. 3,590 of 24 January, 1881.

Admiralty, 1st *Sept.*, 1881.

Mr. E. A. Cowper: My Lord, we all know that there is steel and steel, and that there is a great variety in the quality of steel; but we have been frequently talking of steel simply as steel, without specifying exactly its quality. Now I do think, that great praise and the thanks of the country are due to the Admiralty for the way in which they took up steel, as soon as they were assured that good mild steel of regular quality could be made, and particularly I would mention Sir Nathaniel Barnaby, because he once said, in this room, that the steel then in use for shipbuilding and marine engineering was not trustworthy, but as soon as he found that the mild steel he asked for and specified could be made of uniform quality and could be trusted, he at once adopted it, in the building of the "Iris" and "Mercury" and following ships, and that greatly conduced to its introduction for general use. I think Lloyd's have followed that up thoroughly by insisting on good quality and certain tests in all cases, and, if an example were required to show how a material can practically be improved in quality by a little attention and testing, we have only to turn to "Portland cement," which used to be simply some grey powdered cement, and as long as it set it was thought to be all right. But now that the Metropolitan Board of Works have for some time insisted on tests, the Portland cement makers make a cement of fully double the strength they used to, and at the same cost. It has been remarked by one speaker that we should be fortunate if we knew as much about the skin of a ship as we did of our own article. Mr. Parker's remarks I have followed with very great interest, and a most interesting discussion has already taken place at the Institution of Civil Engineers on the treatment of steel. I think the diagrams of Mr. Parker, showing the several effects of the treatment of steel, are most instructive. I do not take alarm at them as one speaker has, and the results are not so dangerous as he thinks. In no case has the steel been spoiled, but in certain cases where it has been worked to a blue heat, there is less elasticity and ductility left in it than there is when it has been annealed or worked cold, but you have a somewhat increased breaking weight, viz., 31 or 32 tons instead of 27, so that the steel has not been spoiled or destroyed, but if it is placed in a ship contiguous to another piece that has been annealed, and a severe strain comes upon them, the harder of the two takes the heavier strain. I think that ought to be very clearly understood, because when one speaker says that if the material will not stand punching, shearing, hammering, furnacing, and so on, in the roughest possible manner, do not use it at all. It is not a question whether steel that has been somewhat or very roughly used, is actually destroyed, but it is a question whether it has been injured, and whether it can be injured without your knowing it. Now when we come to such precise experiments as those of Mr. Parker, we really do know how it is affected, and how far it has been altered in character, it, therefore, seems but reasonable so to treat it as to avoid all injury. It is a very satisfactory thing to have such a splendid material to use that will bear rough treatment, but, if it had not been for the experiments and tests applied on the introduction

of this mild steel, it would not so soon have attained the character it now bears, and which cause it to be so trusted. Now, I say, Gentlemen, let us continue that as far as is reasonable and right, and we shall continue not only to have the same steel as we have now, but better steel and more regular; we shall then keep bad plates out of our boilers and ships. Another strong argument that will have its weight with shipowners is, that the better the steel and the mode in which it is treated, the greater will be the carrying capacity of the ship, and the very large quantities of mild steel that are now being used by such firms as Messrs. Denny & Co., and many others, point to the probability of our building for America as well as for ourselves, as there has been an alteration in the law with regard to the purchase of ships in the United States.

Mr. J. H. Biles: My Lord, I was very pleased to hear such an eminent authority as Mr. White speaking in such a lucid manner upon the question of the higher tension steels. I think that this meeting, as Mr. Riley has expressed it, might well bring us to the stage when we can say that the steel we are using now, the mild steel, is a material which can be used with as much confidence as was given to iron, and, therefore, I think the question is worth considering, whether we are not now at that stage relatively to mild steel, at which we were ten years ago in relation to iron, and whether we should not now adopt in ships steel of higher tension and correspondingly reduced scantlings. For, we have it proved on the authority of Mr. Martell that the 20 per cent. reduction which is given to the mild steel of from 27 to 31 tons produces a satisfactory result. If, therefore, we can go up to steel of from 31 to 36 tons, we should look for a further corresponding reduction. Of course there are some considerations which come in which tend to modify these reductions, but I think that, stated as a general case, advantage might be taken of the higher tension steel. In connection with this, I may say we are at present building a vessel, the steel of which has a tension of from 32 to 36 tons, and I have had a series of experiments made upon that steel in order to test its working qualities, one of which was to test the effect of galvanising upon it, as all the material is galvanised. I can quite corroborate the experiments which have been carried out by Mr. Ward, and can go further and say, that galvanising steel of even such a high tension as 36 tons improves it. The effect of tempering upon that steel—I do not mean the quenching, but I mean the tempering, which is brought upon it by putting hot rivets in it—is not prejudicial. It has been raised as a strong objection to the use of higher tension steels, that the hot rivets would tend to temper the steel in the vicinity of the rivet holes. From the results of my experiments this does not appear to be the case, there is one point that it may be interesting to some members of this institution to know, and it is in connection with galvanising, though the steel is improved I find by experiment that it does not improve the rivets to galvanise them, but that on the contrary there is very considerable loss of strength if you galvanise rivets before you hammer them up. Of course that applies wholly to rivets which are used cold. With reference to a remark of Mr. Denny's as to the thickness of butt straps in steel ships, and also that the riveting is somewhat excessive, I think that a method which has been proposed by Mr. Mumford, one of Lloyd's surveyors, is worth bringing to the attention of this Institution. It is one I have had much pleasure in making use of, in a vessel we are building. It is, instead of thickening up the butt straps, to put an angle iron in the middle of the strap in a fore and aft direction. This increases the stiffening without anything like a corresponding increase of weight, and you do not run

into such a large amount of riveting for it. This method in my opinion is much preferable to that of thickening the strap.

Mr. JOHN: My Lord, I will only detain the meeting a few minutes, and will only refer to one or two points which have arisen in the discussion to-day. Mr. Parker has mentioned my name in connection with the experiments we made together on the punching of steel plates, and the extent to which the steel lost in strength between the punched holes. Mr. Reynolds also mentioned the same matter, and suggested that if the punching could be done in the thick plates slowly, proportionately as slowly as in the thin plates, so as to give the metal an opportunity of flowing, that the thicker steel plates would not suffer so much as they now appear to do. I quite agree to a large extent with that view, but it is quite impossible in a shipyard to regulate the velocity of your punching machines to each thickness of plate. In the experiments Mr. Parker referred to we find some points of this kind. It was generally believed in those days that the damage done by punching was due to close dies, and that, with open dies, allowing the metal to get away, the injury would cease. We tried plates of different thicknesses, punched some with close dies and some with open dies, and we found, in spite of every precaution we could take, that the thick plates lost tremendously through punching, more, indeed, than I would have believed at the time, if I had not seen it myself. I have been much interested in what Mr. Riley said just now about the rolling of steel plates in an over-heated condition. I think there is a good deal of truth in that. There is one other point I should like to mention in reference to the subject on hand, and that is in connection with corrosion. I have seen myself some very curious instances of corrosion of steel ships, and in our own neighbourhood I have seen a steel ship pitted within six weeks of her being launched. I described that case in a Paper I read before the Iron and Steel Institute two or three years ago. Quite recently I saw another instance of corrosion in a steel steamer, but of a different kind. This was corrosion in butts that had been strained, and the leakage through the butts had set up a corrosive action, and made openings in the butts that you could stick your knife into. It was a question that we discussed, whether those butts had been originally defective, whether they had been strained, or whether it was corrosion due to the straining. We hunted about, and found many such places in the butts, which had been originally planed, that were evidently the work of corrosion. You could not only put a testing knife, but an ordinary pocket-knife into the butts. I had not seen anything similar in an iron ship, and, as I have seen it so recently in a steel ship, I thought it might be interesting to the meeting. I should recommend my friend Mr. Martell to look out for that sort of evidence in steel ships, as a part and parcel of what Mr. Denny calls morbid anatomy of ship construction.

Mr. B. MARTELL: My Lord, I can only express my thanks first for the reception which my Paper has met, and the remarks I have to make will only be very few, seeing the long time that has been occupied in this discussion. I should like first to deal with those subjects where the action which has been taken by Lloyd's Register has been criticised by Mr. Ward and Mr. Denny rather severely; and first, with regard to the question of the increased riveting of the butts of steel ships. Some time ago there was a rumour about, that steel ships were weak, and in fact some people appeared to think that they were dangerously weak. This fact was brought before the committee of Lloyd's Register, and I

was instructed to consider the question fully, and obtain all the information I could with reference to this subject. I not only wrote to all the surveyors of Lloyd's Register, but I placed myself in communication with the principal shipbuilders who had been building steel ships; and I may say I received from nearly everyone I communicated with, the opinion that it was desirable to increase the riveting of the butts. I found from my own experience, and the experience of many of my colleagues, and of the builders I consulted, that there was really no necessity to make any general addition to the scantlings of steel ships. They were found to be doing their work well. But I could not ignore the opinion of those great experienced shipbuilders, particularly those on the Clyde, who advocated that the strength of the fastenings of the butts should be increased, instead of increasing the scantlings, and thereby adding unnecessarily to the weight of the ships throughout. I took into consideration how the butts could best be increased in rigidity, and more particularly with regard to the large ships. There were two ways of doing this: one was by increasing the thickness of the butt straps; and another was by increasing the number of rivets, and I may say that I believe my friend Mr. Denny advocated that himself, though now he criticises the action which has been taken by Lloyd's Register.

Mr. Denny: My Lord, will you permit me one word of explanation. In making my remarks I said we had originally filled in the additional rows of rivets, but that we gave this up several years ago. We found the additional rivets unnecessary, and we have since successfully built ships over 400 feet long without them. But the change which Lloyd's have made is a very recent one, having occurred within the last year. We only used the full rows of steel rivets several years ago, and in one steamer as I have told you we gave up the practice, and I do hope Lloyd's will find out that what is unnecessary in a steamer over 400 feet long, is totally unnecessary in a steamer 315 feet long.

Mr. Martell: I may say, my Lord, that I am not referring to remarks recently made by Mr. Denny here, but I am referring to a letter which he wrote to me in answer to a letter I wrote to him with reference to his experience of steel ships, and I am under the impression—I am open to correction if I am wrong—that Mr. Denny advocated the strengthening of the butt fastenings. But how is it to be done? I am quite certain Mr. Denny recommended thickening of the butt straps.

Mr. Denny: That is correct.

The President: That letter is not before the meeting.

Mr. Martell: I am sure Mr. Denny will not object to my stating this. He is always so generous in his remarks, and so open minded to any criticism himself, that I am sure he will allow me to reply to any criticism he has made. Well, now, there is one great consideration, that if you increase the thickness of the butt straps in very large ships, you get a very thick strap, and it is very necessary in order to get all the strength out of the strap, that you should have sufficient rivets to ply it up well to the plate, so as to get all the benefit out of the strap as well as the plates. We found by experience that only half the number of rivets between those back rows was insufficient. Shipowners like to save expense as much as possible, and they generally manage to leave out a rivet at each end which might have been put in. We found by allowing half rivets in the way we allowed before, and our rules admitted, we could not possibly ply up those thick butt straps, but we required the

additional number of rivets in order to secure them properly. A number of other gentlemen whom I see around me recommended precisely the same thing. Some of them who have built these very large ships themselves, have given me this recommendation : " Do not increase the scantlings, but do increase the fastenings of the butts ; put the rivets closer together, and give us a greater number of rivets for steel ships." I believe I have heard Mr. Denny in this hall recommend that rivets for steel ships should be spaced more closely than in iron ships. Having all these opinions before us we felt it necessary, not only on account of that, but from our own experience, to increase the butt fastenings of those very large ships, and we did this at the least possible expense to shipbuilders, and to the shipowners as we thought. But unfortunately it is very difficult for those who make rules to be able to satisfy everybody. There is my friend Mr. Ward. He says : " Why not compel every shipbuilder to use steel rivets ; " and then another shipbuilder comes and says : " Why are your rules so absolute and tyrannical that you will insist on these things ; you do not give shipbuilders and shipowners any scope to do what they please ? " So that really whether we do a thing or whether we let it alone we are always getting into the wrong. Now, I may say that there are a great many shipbuilders who object to steel rivets. I do not myself. When a shipbuilder comes to me and asks my opinion, I say : " Use steel rivets by all means." We have now a quality of soft steel that can be used with the greatest certainty. I therefore say utilize the steel and get the greatest amount of benefit out of it. Still there is something more to be said with regard to this point. What is the objection to iron rivets ? I have seen many ships which have broken down, and I have never seen the rivets shear where a vessel has broken asunder. The rivet area is always sufficient. The butt strap has either given way, or the plate has given way through the rivet holes, or the plate has broken right through the solid part ; but I have never seen a ship go to pieces owing to the shearing of the rivets. That shows that even the iron rivets are strong enough, and that with iron rivets you get a sufficiently strong ship, and satisfy those who want to adopt iron rivets. Mr. Ward wishes Lloyd's Committee to be more tyrannical than they are now, and to make this matter absolute, but they like to leave a little to shipowners and shipbuilders. There is no doubt that as Mr. Ward says, you theoretically reduce the longitudinal strength of the butt doing this. That you do theoretically I admit, but in practice the great additional advantage that you get by laying up the butt strap properly, and getting all the great advantage of the frictional surface out of that strap, far more than compensates for the little deficiency in theoretical strength you get by calculating it in the way Mr. Ward has done. It strikes me as being somewhat contradictory what Mr. Ward said in regard to this question of annealing. Mr. Ward in one place says, in regard to annealing, " these tests shew that either the annealing effect of a hot rivet counteracts the injury that is done to the steel, or that the injury is purely imaginary." In the next paragraph he entirely objects to the benficial effect of annealing ; so that even supposing any advantage to be gained by the hot rivet, which I certainly have very great doubt about indeed, for I do not think a hot rivet is going to make any difference to the steel, it is very strange that some good effect should be attributed to it in one paragraph, and that in the next paragraph it should be entirely dissented from, and said to be entirely useless. I believe that those experiments which Mr. Parker has conducted with so much intelligence and ability are very important indeed, as shewing the effect of annealing on plates. With regard to the steel ship I have mentioned, I may say that Mr. Denny suggested the water was played on to the plates through a hose while they were red hot, and the water cooled them in one part,

while they remained heated in another. But I want to know what difference there is between local cooling and local heating? It was that which altered the temperature and the quality of the plate, and it was that which caused it to break down in the manner in which it did. I was very pleased to hear Sir Nathaniel Barnaby and Mr. Cowper speak as to the care which is necessary to be taken with regard to steel. I think in respect to all these successes that have been claimed by Mr. Ward and Mr. Denny, and others, with regard to steel ships, Lloyd's Register must take a good deal of the credit to themselves, as a society, for the care that they have always exercised in seeing that proper tests have been insisted on, and that the steel has been produced of an uniform quality. I am quite convinced of this, that had we not insisted on the tests we did, we should not have had steel in the same satisfactory condition that it is in at the present time. I have heard Mr. Denny say here with all his enthusiasm: "Do not give up your tests, but if possible make them more stringent;" and I agree with that most thoroughly. That likewise entirely agrees with the views of the most intelligent steel manufacturers. Mr. Riley has said: "It is the best protection we can have for you to insist on rigid tests. Never give them up, because, if you do, all the inferior manufacturers will come in against us, and compete successfully with us, and we shall get indifferent steel." So that I think we must still insist on that careful examination of this material. We must go on in the way in which we have been going, which has resulted in such beneficial effects, and has produced such magnificent ships, and increased the confidence of shipowners. So long as we continue our tests I am sure that steel ships will increase in tonnage in this country, and will supersede iron, as I have said before; but it will be a leap in the dark if we ever give up the restrictions that we are compelled to put upon the production of this material—this rigid examination and these rigid tests. I will not keep you many minutes longer, my Lord. I intended to take the remarks of the speakers *seriatim*, but really I find that the remarks are so long, and there are so many of them, that I am afraid that time will not permit me to do so. I do not agree with Mr. Denny at all with reference to his remark regarding ships leaking through the butts if a knife cannot be got into them. He thinks that a surveyor who cannot see the butts might assume that the leakage is not through them, and that the ship is not strained, as I understood him. I can only say with regard to the case I mentioned I saw it myself; and therefore when I tell what I saw from my own experience I expect my statement to be accepted in the same manner, and with the same kind of generous acceptance with which I should accept any statement of Mr. Denny's. I can easily conceive that a ship lying on the blocks, perfectly upright, might not show any indication of leakage, and that you could not see the slightest indication of any cracking of the butts which would admit water. But yet when a ship is labouring in a heavy sea and when she is heeling over so that the side becomes the top of the girder, instead of the gunwale, the transverse lateral movement of the ship would tend to open the butts, although they may not be seen when she is upright and lying on the blocks. I should like to say more, my Lord, but time will not admit.

P.S.—"Since making the above remarks, I have had some correspondence with Mr. William Denny, who demurs to the statement that he advocated increased rivet area. On reference to the letters to which I alluded, received from shipbuilders on the Clyde, I find that though nearly all of them advocated this, Mr. William Denny did not. He, as will be seen from the following extracts, recommended as a remedy for the complaints about steel butts, which he anticipated, increased

thickness of butt straps and improved workmanship. The following are the extracts from his letter above referred to bearing on this subject:—'. . . I think, if I am not mistaken, my firm was the first to introduce the practice of paying extra attention to the butts in steel steamers, both in the way of giving them much thicker butt straps in proportion than we allow in iron steamers, and also in taking care that the butts should be practically tight without caulking, and only receive a touch of the caulking tool as a matter of finish. . . . *Rivets.*—We are of opinion that all steel steamers should, at least in the shell and such longitudinal strengthenings as stringers and steel decks, be riveted with homogeneous steel rivets. We have found these rivets to be very reliable. We are against the use of iron rivets, both because we consider them inferior in the event of the steamer being subjected to heavy blows in collision or in going ashore, and also because we think there is reason to believe they have a tendency to lead to corrosion. . . . The quality of workmanship which is quite sufficient for an iron steamer will be found quite inadequate for building a steel steamer, if she is not to be a source of constant repairs to her owners. In the matter of butts alone, I should say that most iron shipyards would require to have their planing machines put into perfect order, so that they could plane a butt in a truly straight line; and also that their platers and riveters should be taught how to make butts which will be absolutely "knife tight" without the touch of a caulking tool. Unless this standard of workmanship is attained, your Society will have any number of steel steamers suffering from defective butts, and the ordinary surveyor will at once jump to the conclusion that the cause is weakness, and not bad workmanship, or, rather, insufficient workmanship. I say insufficient workmanship, because the workmanship which is at present in vogue for iron steamers, and is sufficient for their scantlings, cannot be called bad so long as it is sufficient. In the matter of scantlings, while we think there should be no addition made to them, we are of opinion that they might be redistributed with some advantage, that some portions might be thickened and others thinned, so as to leave the total weight of steel as at present, but differentiated. Unquestionably thicker butt straps, in proportion to the thickness of the plates, are required in steel vessels than are required in iron vessels, and, perhaps, also somewhat thicker plates about the heel, unless special provision in the way of double transoms and moderate shinty footing are made.'"

Mr. J. Ward: My Lord and Gentlemen, I have to thank you for the frank criticism that has been given to my Paper, and for the compliment you paid me in supposing that in some mysterious manner an electric current of influence had in these two Papers passed between Mr. Martell and myself. Any relationship with Mr. Martell is honourable, but in this case the compliment is undeserved. With regard to Mr. Martell's remarks, he states in connection with the increase of riveting, in the butts of steel steamers, that he has asked the whole of his surveyors and the leading shipbuilders to give him their opinion, and that the result of their opinion was the recent alterations embodied in the last circular issued to shipbuilders. I am perfectly at one with Mr. Martell in his views about thickness of butt straps, and also in getting more rivet area by increasing the pitch from four to three and a-half diameters, as he has done, which is in itself a very considerable increase, but I still say that in postage-stamping a butt to the extent he does by insisting upon three complete rows of rivets, he is really injuring the ship to a very serious extent and not increasing her efficiency. Mr. Martell, although he states that he has known no ship fall to pieces riveted with iron rivets,

does not tell us whether in the steel steamers which presumably showed signs of weakness, this did not arise from their being riveted with iron rivets. I hold that my argument regarding iron rivets is perfectly fair. I have brought forward to-day fifty-two steamers running in four of the principal British steamship companies, grossing 153,000 tons, and I have the reliable testimony of their superintendents that in every case these steamers (ranging from 250 to 420 feet in length) all of them, with few exceptions, riveted with steel rivets and with the outer rows incomplete, have in no single instance shown defective butts. I say that fact ought to have as much recognition in inducing Mr. Martell to reconsider his decision as the opinions which he told us he has had. Mr. Martell says he cannot please everybody about rivets, but nobody expects that, until he becomes infallible, he will please everybody on this or other points. Please note that these rivet diagrams which are marked "good iron," "bad iron," and "very bad iron," are not correct; they should be marked instead "good fracture," "bad fracture," and "very bad fracture." The iron rivets were tried before being used, and were successfully bent cold. The quality was undoubtedly good. These riveting tests (*see* Appendix) dealt with nothing save the riveting up of the plates, caulking them, and immersing them in water, the rivets being tested before they were put in water, and before they were cut out. The plates were immersed for a fortnight. All the fractures tabulated are the result of contraction in the working and subsequent cooling, so that any signs Mr. Martell has seen of weakness in steel steamers which were iron riveted as far as the butts are concerned, may be due to a large percentage of fractured rivets which would not be found in steel riveted butts. This is a point which, however it may at present be ignored, will become more and more evident as experience grows. Apart from our own extensive tests, one of the leading shipbuilding firms on the Clyde, who are at present engaged in Admiralty work, carried out a similar series of tests. The results were, that in the case of the steel rivets, without exception, there was not a single fracture, but 50 per cent. of the iron rivets showed fractures in one series of plates, and in a second set 90 per cent. of the iron rivets were fractured. That is a revelation which you must realize, and with all the importance which these facts demand. I have always understood that an essential feature in the proper arrangement of treble butt fastenings was that the number of rivets in the row furthest from the butt should, if possible, not exceed the number through the frame. Take one result of the present system of riveting as per Lloyd's recent circular; in a steamer we are now building with a sheer strake 48 inches broad and having the third row in the butts complete as now required by Lloyd's Rules, the shearing strength of the rivets is 988, while the strength of the plate at the outer row is reduced to 622 tons as compared with a strength at the frame rivets of 822 tons. Surely this is a needless weakening of the sheer strake, and I cannot conceive anything which warrants such a practice. If you take an ordinary shell butt, the comparison is as bad; but, my Lord, instead of reading the calculations, I shall, with your permission, append them.* Instead of gaining by the increase of the rivets in the butt strap, the ship has been weakened to a very material extent. On the other hand, the vessels riveted with steel rivets according to Lloyd's original Rules, regarding which I have read you the results of reliable experience, prove that there is no warrant for the sweeping alterations lately made by Lloyd's Register. Mr. Martell speaks about annealing, about some tests I have made, and suggests that I ignore them and make other assumptions in a paragraph or two afterwards. If Mr. Martell had

* *See* Table, p. 151.

taken the trouble to read my Paper with care, he would have seen that I put forward some propositions of which he may not be altogether ignorant; propositions which were put very lately, but not by me. In the tests I made of plates subjected to two and three rivetings and cuttings out, I give results called for by the assumptions involved in these propositions. We have a choice of one of two things, either that these propositions were abstract and unimportant, or that there was some supposed truth in them. The results we got by testing mean again one of two things, either that the hot rivet did anneal the plate or that, as Mr. Martell says he believes, and as I believe, the supposed injury existed only in the imagination. There is no contradiction in this. As to Mr. Parker's remarks, let me say that I wish in no degree to curtail or to diminish any tests that have ever been required from steel makers, and I say this with as great an experience, as intimate a knowledge, and as deep an interest in steel as any member here. We believed in steel and worked it when there were not many who did either, and we do not need any one to teach us the importance of the tests which guarantee its reliability. I have every desire for thorough testing; the more the better, especially at the works. When the steel arrives in the shipyard, you ought to have this guarantee, that the material you receive is uniformly good and reliable. Starting from that point, I have given you the results of ordinary shipyard treatment. The steel must get that treatment, as you cannot stand by men all the day and prevent it. They will treat it as they have done iron, and you may as well face the fact, nor do I think any of the steel makers would wish it to get a different treatment. As to blue heat, I ought to mention that I do not mean you to wait until a plate cools and then to give it the maximum amount of work. The greater portion of the work is done at a red heat, but if a few more blows are required to complete the work, the men will certainly not stop, and it will get these at the blue heat; happily, as our experience shows, with only the rarest injury. It is far better to realise all this and to face a few rare failures under these conditions, than to have your men pretending they are carrying out impossible conditions. I believe such failures as do occur are not so much the fault of ordinary working, but are due quite as much to imperfect material. I realise, especially in steel as in iron, for sail or steam ship construction, that its purpose is not merely for the carrying of merchandise, but beyond that for the safety of human lives. Feeling all this, I believe that working the material you get in the shipyard free from all coddling restrictions is the best safeguard you can have for the discovery of defects, and for its power to fulfil the conditions and risks required in navigation.

The President: Gentlemen, I am quite sure you will allow me to convey your united thanks for and approbation of these two valuable Papers, and I am sure also that the authors of these Papers will be well satisfied with the very thorough discussion which has followed upon them. Mr. Martell's complaint, gentlemen, that he cannot please everybody, reminds me of a little story which I heard the late Sir James Graham tell a committee on which I was sitting. A drummer boy was once flogging a soldier and he hit him high up on the shoulders. The soldier holloa'd, and said, "hit low, that hurts." The drummer boy hit him low and the soldier holloa'd again, and then the drummer boy turned round and said to the officer, "You see whether I hits him high or whether I hits him low I cannot please this fellow no how." That seems to me to be very much the position of our friend Mr. Martell, and I wish him joy of that position.

COMPARATIVE STRENGTH OF TREBLE RIVETED BUTTS. PLATE AND STRAP 40 INS. WIDE.

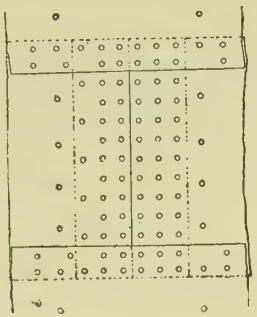
COUNTERSINK OF HOLES DEDUCTED FROM SECTIONAL AREA OF PLATES. RIVETS TAKEN AT ORIGINAL DIAMETER.

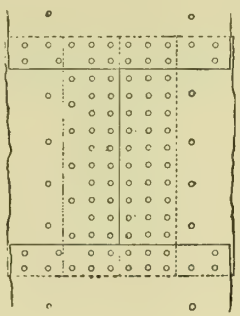
Material.	Spacing of Rivets.	Riveting of 3rd Row.	Shearing strength of Rivets.	Strength of weakest section of plate at butt + 3rd row of Rivets, if incomplete.	Strength of plate at frame riveting.	Relative Strength. Strength of plate at frame being taken = 100.	
						Riveting.	Plate.
½ in. steel plate	4 diameters.	Incomplete	370 tons	433 tons	468 tons	79·1	92·5
		Complete	437 tons	355 tons		93·4	75·9
⅞ in. iron rivets	3½ diameters.	Incomplete	403 tons	428 tons		86·1	91·5
		Complete	470 tons	338 tons		100·4	72·2
½ in. steel plate	4 diameters.	Incomplete	505 tons	462 tons	468 tons	107·9	98·7
		Complete	597 tons	355 tons		127·6	75·9
⅞ in. steel rivets . . .	3½ diameters.	Incomplete	550 tons	460 tons		117·5	98·3
		Complete	642 tons	338 tons		137·2	72·2
½ in. iron plate	4 diameters.	Incomplete	360 tons	390 tons	365 tons	98·6	106·8
¾ in. iron rivets		Complete	420 tons	290 tons		115·1	79·5

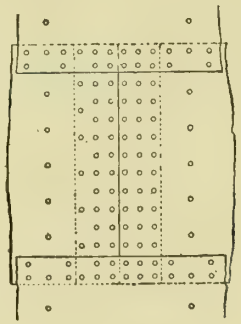
STRENGTH OF MATERIALS USED.

From Mr. Wildish's Paper in Transactions of the Institution of Naval Architects for the year 1885.

Shearing of Iron Rivet in Iron plate = 22·6 tons per square inch
" " " Steel " = 18·6 " "
" Steel " Steel " = 25·5 " "
Tensile strength of Iron plate = 22· " "
" " Steel " = 28·2 " "







ON THE USE OF STEEL CASTINGS IN LIEU OF IRON FORGINGS, AND BRASS CASTINGS IN BUILDING AND FITTING SHIPS.

By E. C. Warren, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 15th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The short Paper which I am about to read will, to a considerable extent, express the united opinion of the Committee upon the use of steel castings for H. M. Service with which I was lately connected; and I have permission to use such portions of that Report as may, in my opinion, be necessary to place this subject fairly before you.

The proper value of steel castings is not sufficiently understood by shipbuilders, engineers, and other manufacturers who work in metals; it seems to me that a more intimate acquaintance with their value for many purposes, and especially as a substitute for iron forgings, and brass castings, and even in lieu of iron castings where strength and lightness have to be considered, will before long bring about a very great change, and the more steel castings are used the more will they be appreciated and sought after; provided that a fair price be paid, and an honest material be supplied by the manufacturer.

Castings in steel are generally made from crucible, or open-hearth steel: but there are some makers who use steel of special manufacture.

The points which I propose to consider are—

1st. What success has, up to the present time, been obtained in producing steel castings for use in lieu of iron forgings.

2nd. What description of articles usually made of forged iron, or brass castings, could with advantage be made of cast steel; having regard to weight, strength and durability.

3rd. What tests should be applied to various descriptions of steel castings.

With regard to the 1st point, I can confidently state that very great success has already

been obtained in producing steel castings in lieu of iron forgings; this opinion has been formed after visiting a large number of the principal steel foundry works in England and Scotland; and in carrying out a series of exhaustive tests especially with small castings. *See* Appendix I. and Plate XVI., which is a sample of one of the series of tests so carried out.

The small articles selected for testing purposes were :—

1. Clips for watertight doors.
2. Eye-plate, 1 in., for use on iron beams, &c.
3. Pivot bar for 20-pounder gun.
4. Clips for securing chains for 6-in. B. L., and other guns.
5. Hooks for tackle blocks.
6. Internal binding for 10-in. snatch block, and swivel hook.
7. Internal binding for 10-in. treble block, swivel eye, and shackle.
8. Deck plate with double eye and links for securing guns.
9. Fighting pivot for 64-pounder and 7-in. guns.
10. Longitudinal intercostal angle 3 in. × 3 in. × $\frac{7}{16}$ in., long 3 ft. 0 in.
11. Bedplate for Nordenfelt gun.
12. Intercostal beam angle 3 in. × 3 in. × $\frac{3}{8}$ in., long 2 ft. 0 in.
13. Intercostal angle 3 in. × 3 in. × $\frac{7}{16}$ in., long 2 ft. 6 in.
14. Boat's crutch.

In addition to these a great number of larger steel castings were examined and tested, viz. :—

a. Stems for ships of various sizes.
b. Stern posts for do.
c. Stern frames for do.
d. Rudders for do.
e. Brackets for supporting after ends of propeller shafts.
f. Dredger chains and buckets.
g. Hawse pipes.
h. Anchors.
j. Frames for gun carriages.
k. Saddles do.
l. Main bearing frames for engines.
m. Crank shafts.
n. Wheels of all sizes.

o. Cylinders.
p. Propeller blades.
q. Rudder frames.
r. Caps for masts and bowsprits.
s. Riding bitts.
t. Capstans.
u. Cable-holders.
v. Deck pipes and plates.

See Appendix II. and Plates XVII., XVIII. and XIX. inclusive, which gives sketches to scale of some of the articles previously enumerated, and also the tests to which they were subjected.

These speak for themselves as to the difficulty of making perfectly sound and reliable castings for the articles shown; but the difficulty existed only to be overcome, and no one could see the splendid specimens of castings which were turned out, and which stood most satisfactory tests, without being convinced that in cast steel we have a material, which, although only partially developed, is of the utmost value. We want the ability and determination of the makers, and the intelligence and skill of the workers combined, to still further develop and perfect this branch of manufacture.

I think a similar process will have to be gone through with steel castings as that which took place with respect to the manufacture of mild steel for plates, &c., a few years since; and which has produced such great results, that mild steel plates, angles, &c., are now produced at prices which were not then thought of, and of such excellent quality and homogeneity, that we may almost say we possess a perfect article at a most moderate price.

When the same process has been passed through for steel castings, a great deal of inferior make will disappear—like the old steel plates which would break up like a sheet of glass if they were let fall upon an iron slab—and we shall get an article in every way reliable, at a greatly reduced price.

It is found that small castings made from good crucible steel are remarkably tough, ductile, and of uniform quality, while many of those made from other steels are very unsatisfactory, and unfit substitutes for small iron forgings.

For large articles, good, sound, reliable castings can be made, with proper care, from either crucible or open-hearth steel.

With regard to the second point, viz., What description of articles usually made of forged iron, or brass castings, could with advantage be made of cast steel, having regard to weight, strength and durability? It is, I think, evident that the question of cost will largely determine the extent to which steel castings are brought into use; for small articles, where only a few of one description are wanted, it will be cheaper, and a saving of time to continue to make them of forged iron; but even for very small fittings, where large numbers of similar patterns are required, the saving by using cast steel will be very considerable, both in time and money.

For larger articles, such as stems, stern-posts, brackets for supporting propeller shafts, rudder frames, racers for guns, frames for water-tight doors, trollies for machine guns, hawse pipes, &c., the saving would be very great indeed.

Appendix III. gives a list of such articles or parts of a ship, or her fittings, which could be advantageously made of cast steel.

The strength of good steel castings is beyond question, and their durability is very great, and the saving in weight is very considerable.

I have seen small steel castings which have been in every respect equal to the best iron forgings,—but I am also bound to say, that I have seen others which could not be safely used on board ship, or in any position where they would be subject to shocks or vibrations; and the greatest care is required in selecting and testing steel castings before they are accepted.

There are steel castings, and steel castings, good, bad, and indifferent, and the price paid will often, to a great extent, determine to which category the articles belong. I am certain that even at the present time a fair price paid to any of our well-known Steel Founders, will enable us to obtain sound and reliable castings, which will bear the most rigid examination and tests.

Too often prices are cut down to such an extent, that the temptation to use inferior materials is irresistible, and when such is the case—the usual results follow, and there is a failure—and thus it is we so often hear of steel castings having failed, and so an undeserved slur is thrown upon the manufacture.

There are some well-known Firms who are too high-minded to allow any but the very best materials to be used, and the result is, that their castings are almost uniformly of the highest character, and none but the best are allowed to leave their works.

It is to makers of this type that we must look for improving and cheapening the cost of steel castings. At the present time every effort is being made by most able and skilful men to investigate the nature and combination of metals, both scientifically and practically, and the trained chemist is standing by to watch, and work out carefully the results ; and it is to be hoped that before long we shall be able to produce perfect steel castings at a most reasonable cost.

This brings us to a most important point of our subject, viz. :—

What tests should be applied to various descriptions of steel castings.

It is extremely difficult to lay down any general law on this point ; steel castings are used for so many purposes, involving entirely different conditions, according to the nature of the work required from them, that the tests which are absolutely necessary to be obtained in some cases, may be dispensed with in others.

In Appendix III. is given the list of fittings proposed to be cast in steel, and also the tests which it is considered should be applied to each.

The appearance of the fractures of broken castings gives almost certain indications of the nature of the materials ; the percussive test is most valuable where it can be applied ; and it is most desirable that when a number of articles, or fittings, of the same, or similar patterns are supplied, a test to destruction should occasionally be made, to obtain information from time to time, as to the ultimate strength of the castings.

I consider it undesirable, and unnecessary, to specify a minimum and maximum tensile strength per inch ; if the minimum be given, and the elongation in a given length, and a bending test, together with a percussive test, where such may be applicable, it is sufficient to ensure perfectly reliable articles.

It is a well-known fact that some steel which will bear a greater tensile strain than the maximum specified, will, notwithstanding, show excellent results of elongation and bending ; and when this is the case, to insist upon its rejection because of its superior strength would be unreasonable ; and as a matter of fact—having obtained the minimum tensile test to ensure sufficient strength—the elongation and bending tests should determine the question as to receipt or rejection.

I should like to make reference briefly to some steel castings which were cast at Stockholm by Nordenfelt's patent process ; they were most interesting, and of superior

finish, and they could be bent and twisted cold in an extraordinary manner without fracture. A small specimen given by Mr. Nordenfelt, with permission to test it in any manner desirable, gave the following results, viz. :—

Tensile test 28 Tons per square inch.
Elongation in 2in.—12·8 per cent.

I am of opinion that if castings by the Nordenfelt process can be produced at a reasonable cost, they will be of very great value in lieu of small iron forgings and brass castings.

It will be observed that I have not dealt with chemical tests for steel castings. I attach very great importance to the labours of the chemist in relation to the manufacturer; but I do not attach any value to the chemical tests which are sometimes insisted upon before castings are accepted by the purchaser.

The large variations in the results of tests obtained by different chemical manipulators are most perplexing, and in my opinion they may be safely neglected—the mechanical tests previously named being amply sufficient to meet every possible requirement.

Appendix I.

Details of Tests of Small Steel Castings representing articles of common use in Forged Iron, or Brass Castings, in Ships of War.

See Plate XVI.

No. on Plate XVI.	Article Tested.	Kind of Steel used.	Description of Test applied.	Remarks.
1	Clips for watertight doors.	Crucible steel.	Bent cold on an anvil by sledge hammers. Not fractured.	Very satisfactory.
1*a*	Do.	Do.	Heated and straightened as shown, then cut in two and welded up, and bent double when hot across the weld.	Proved to possess good welding properties. Very satisfactory.
2	Eye-plate as used for iron beams, &c.	Crucible steel.	The eye bent cold through an angle of 45° by blows from a sledge hammer. No fracture.	Very satisfactory.
3	Pivot-bar for a 20-pr. gun.	Crucible steel	One end of the arc of the bar was placed on an anvil and the other struck by a small steam hammer, it was distorted as shown on the sketch before any fracture took place.	Very satisfactory.
			It took four blows of a 22 cwt. weight falling through a height of 4 ft. to fracture the straight part of the pivot-bar.	Very satisfactory.

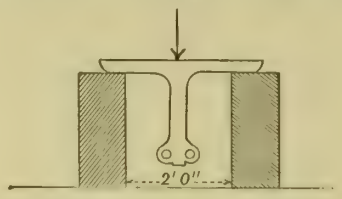
Appendix I.

No. on Plate XVI.	Article Tested.	Kind of Steel used.	Description of Test applied.	Remarks.
4	Clips for securing chains of guns.	Crucible steel.	Bent on an anvil by a steam hammer, as shown on sketch, without fracture.	Very satisfactory.
5	Hooks for tackle blocks.	Crucible steel.	The hook was placed on an anvil, and the point closed as shown by a steam hammer. No fracture.	Very satisfactory.
6	Internal binding for 10 in. snatch block.	Crucible steel.	The binding was placed in a vice, and opened out by a lever, slight fracture in angle as shown. Another binding, hook, and pin, were then placed in a testing machine, and tested up to 12·277 tons, when the binding became distorted round the pin, and the hook broke.	Very satisfactory. Hook took a permanent set at 6·071 tons. Very satisfactory. Pins of blocks of wrought steel.
7	Internal binding for 10-in. treble block.	Crucible steel.	The binding was bent on an anvil by a steam hammer, as shown on the sketch. No fracture. Another binding, hook, &c., were then placed in a testing machine, and tested up to 8·035 tons. Hook fractured.	Very satisfactory. Satisfactory. Pins of blocks of wrought steel.
8	Deck-plate with double eyes and links for securing guns.	Crucible steel.	This article was placed on a hollow iron bed, and subjected to blows from a falling weight of 22 cwt., viz. :— 1st blow, 2 ft. fall. Two corners broke off owing to unequal bearing in hollow plate. *See* sketch. ft. 2nd blow - 2 fall. No further damage. 3rd ,, - 2 ,, ,, 4th ,, - 6 ,, ,, 5th ,, - 10 ,, ,, 6th ,, - 22 ,, ,, At the last blow a slight crack was observed in one eye, and one in the middle of plate. *See* sketch *a*. 7th blow - 22 ft. fall Eyes and links broken. *See* sketch *b*.	Very satisfactory.
	Link out of a deck-plate as above, 6 in. extreme length 1·26 in. diameter.	Crucible steel.	Elongated .2 in. in the testing machine, and broke at a strain of 27·499 tons.	Very satisfactory.
9	Fighting pivot for a 64-pr. gun.	Crucible steel.	Subjected to a test from a falling weight of 22 cwt. in the direction of the arrow, the pivot resting on an iron plate. ft. 1st blow . . . . 2 fall. 2nd ,, ... 4 ,, 3rd ,, ... 4 ,, No damage beyond bending point of pivot, as shown in sketch.	Very satisfactory.

Appendix I.

No. on Plate XVI.	Articles tested.	Kind of Steel used.	Description of Test applied.	Remarks.
10	Longitudinal intercostal angle 3 in. × 3 in. × $\frac{1}{16}$ in., long 3 ft.	Crucible steel.	Subjected to blows from a falling weight of 22 cwt. on the edge A.— ft. 1st blow . . . . 1 fall. 2nd ,, ... 2 ,, 3rd ,, ... 2 ,, At 3rd blow fractured at a slight fault in edge of casting. *See* sketch *a*. Another intercostal was similarly treated.	Very satisfactory.
			ft. 1st blow . . . . 2 fall. 2nd ,, ... 2 ,, Slight fracture on one edge. 3rd blow . . . . 2 ft. fall. Closed without further damage. It was then struck on one corner by the same weight.	Very satisfactory.
			ft. 1st blow . . . . 6 fall. 2nd ,, ... 10 ,, Fractured. *See* sketch *b*.	
11	Bed-plate for Nordenfelt guns.	Crucible steel.	This article was placed on a hollow iron casting, the pins resting on the sides of the hollow; it was subjected to blows from a 22 cwt. weight of varying heights, until at 22 ft. the pins were broken off. A second blow at 22 ft. fractured the edge.	Very satisfactory.
12	Intercostal beam angle 3 in. × 3 in. × $\frac{3}{8}$ in., long 2 ft.	Crucible steel.	This was placed on an anvil under a small steam hammer, and the angle was flattened completely, and the ends distorted as shown on the sketch. Not fractured.	Very satisfactory.
13	Intercostal angle 3 in. × 3 in. × $\frac{1}{16}$ in., long 2 ft. 6 in.	Crucible steel.	One end rested on an iron plate, and a weight of 22 cwt. was let fall upon the other end.	Very satisfactory.
			ft. 1st blow . . . . 1 fall. 2nd ,, ... 2 ,, Closed $3\frac{1}{2}$ in. No fracture. The intercostal was then placed with the edge A on a plate, and a blow was struck at B from a height of 5 ft. *See* sketch *a*. No fracture.	Very satisfactory.
			Another intercostal was subjected to blows from a steam hammer, the angle being flattened as shown in sketch *b*. It was then further distorted as shown in sketch *c*. Slightly fractured at root of angle.	Very satisfactory.

APPENDIX I.

No. on Plate XVI.	Article Tested.	Kind of Steel used.	Description of Test applied.	Remarks.
14	Boat's crutch pinnance.	Crucible steel.	The foot of the crutch was placed on some baulks of timber as shown, and subjected to blows at point marked by arrow from a falling weight of 22 cwt.  1st blow . . . . 8 fall. 2nd ,, ... 12 ,, 1st blow deflection . $1\frac{3}{8}$ in. 2nd blow fractured.	
			An arm of the crutch was straightened cold by a small hydraulic jack. Not fractured.	Very satisfactory.
	Caulking test . .		An intercostal angle with one edge chipped was riveted to a plate, and was caulked with ordinary caulking tools.	Very satisfactory.

Upon trial this steel was found to weld easily.
The average tests of samples gave as follows:

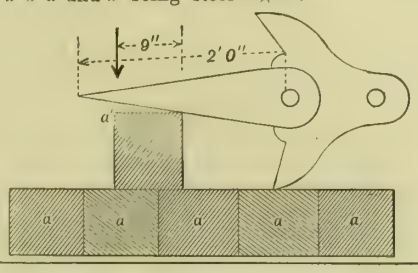
Tensile test 28·932 tons per square inch.
Elongation test in 2 in. 21·35 per cent.
Bending test 2 in. × $\frac{3}{8}$ in. 82°.

APPENDIX II.

A Statement of some of the large Steel Castings which have been made, and tested under various conditions, at various Works, distinguishing in each case the description of steel used and the tests applied.

No. of Plate XVII.	Casting.	Kind of Steel used.	Description of Tests applied.	Remarks.
2	Stem for a foreign war vessel.	Open-hearth steel.	Falling test through 60°, *i.e.*, the upper end of each piece of stem was raised, while the lower end rested on the ground, until the side of the stem above made an angle of 60° with the ground; it was then let fall. The casting was then slung, and sounded with hammers.	Examined and found perfectly sound. Satisfactory.
3	Stem for a foreign war vessel, with under-water torpedo tube.	Open-hearth steel.	This casting was not subjected to a drop test. Test pieces gave the following results:— Tensile per sq. in. 36 tons. Elongation in 8 ins., 12·5 per cent. Bent, cold, $1\frac{3}{4}$ in. × $\frac{1}{2}$ ft — 42°.	
4	Stern post for a foreign war vessel.	Open-hearth steel.	Each piece was raised bodily 10 ft. and allowed to drop on hard ground. Afterwards slung, and sounded with hammers. Test pieces gave the following results:— Tensile per sq. in. 31 tons. Elongation in 8 in., 20 per cent. Cold bend, 60°.	Results satisfactory. Very satisfactory.

APPENDIX II.

No. on Plates XVII. to XIX.	Casting.	Kind of Steel used.	Description of Test applied.	Remarks.
5	Stern post for H.M.S. "Forth."	Crucible steel.	1st. The keel piece ZY resting on the ground, the upper part was raised to an angle of 60°, and then let fall. 2nd. The points X, Y resting on the ground, the post was lifted at Z to an angle of 60°, and then let fall. The casting was then slung, and sounded with hammers. Sample tested :— Tensile per. sq. in., 30·5 tons. Elongation in 6 in. 12·5 pe cent. Bent cold, ·735 in. diar. to an angle of 98°.	Very satisfactory.
6	Balanced solid rudder for a foreign war vessel.	Crucible steel.	Test pieces :— Tensile per sq. in. 30·61 tons. Elongation in 8 ins. 13·67 per cent.	
7	Stern frame and solid rudder, for a merchant vessel.	Crucible steel.	Tensile per sq. in. 31·09 tons. 33·8 ,, 32·00 ,, Elongation in 3 in. 28·5 per cent. 23·95 ,, 15·18 ,, Bent cold $1\frac{1}{8}$ in. × $1\frac{1}{8}$ in. through an angle of 122° before breaking.	Satisfactory
8	Brackets for supporting after ends of propeller shafts for H. M. S. "Benbow."	Crucible	Falling test :— Raised to an angle of 60° : 1st. With ends of arms on ground, boss raised. 2nd. With one arm on the ground, the other raised. 3rd. The opposite to 2nd. And from these positions allowed to fall on the ground. Slung, and sounded with hammers. No defects. Test pieces :— Tensile per sq. in. { 25·507 tons. / 26·065 ,, / 27·681 ,, / 26 746 ,, } Elongation per cent. in. :— 8 ins. ... ... 15·63 8 ,, ... 24·21 6 ,, ... 19·79 6 ,, ... 16·66 Bent cold :— ·75 in. diar. ... ... 180° 1 in. × 1 in.... ... ... 85° 1 ,, × 1 ,, ... 140° 1 ,, × 1 ,, ... 140°	Satisfactory. Satisfactory.
9	Anchor weighing 10 cwt.	Special steel.	One arm A was let fall from a height upon rigid steel slabs. 1st fall, 10 ft. No damage. 2nd fall, 17 ft. No damage. Then placed thus : *a a a a* and *a'* being steel ingots.  	Results very satisfactory.

Appendix II.

No. on Plate XIX.	Casting.	Kind of Test used.	Description of Test applied.	Remarks.
10	Saddle for gun-carriage,	Open hearth steel.	Tons. Tensile per sq. in. 28·81 29·2 Per cent. Elongation in 2 in. 24 30	
11	Main bearing frame for marine engine.	Open hearth steel,	Not any tests applied.	
12	Crown wheel	Steel made by special process.	Let fall from a height of 12 ft. upon hard ground. No damage. Test piece from worm and crown wheels gave— Tons. Tensile per sq. in. ... ... 37·988 Elongation in 4 in. ... ... 10·94 %	Satisfactory.
13	Deck-plate	Steel made by special process,	Falling test as for worm and crown wheels. No damage. Test pieces gave— Tons. Tensile per sq. in. ... . 27·50 Elongation in 2 in. ... ... 6·20 %	This was a very severe test, but the casting stood it well. Very satisfactory.
14	Capstan body found unfit for H.M.'s Service, owing to defects in casting.	Steel made by special process.	This was let fall from a height of 12 ft. upon hard ground. No damage. The first capstan body cast was found to be defective when taken out of the sand. This the Committee were allowed to test in any way they thought proper. 1st. It was let fall from a height of 16 ft. upon hard ground. No damage. 2nd. It was let fall from a height of 10 ft. upon a bed of steel ingots, when the flange was fractured in the place where the defect was observed and for which it was condemned. It was again let fall upon the steel bed from a height of 15 ft. 6 in., when a portion of the flange was broken off. Tested in machine— Tensile, per inch, 26·936. Elongation in 2 in., 6·250 per cent.	Very satisfactory. Very good result for such an intricate casting, and with known defects.
	Bar or drum-head .	Steel made by special process.	Let fall from a height of 12 ft. upon hard ground. No damage. Test pieces— Tons. Tensile per sq. in. . . 30·00 Elongation in $\frac{1}{2}$ in. . 12·5 per cent.	Satisfactory.

APPENDIX III.

A List of such Articles or Parts of Ship, or her Fittings, which could be advantageously made of Cast Steel, together with the Tests it is proposed to apply to each.

No.	Article.	Description of Test proposed to be applied.			
		Minimum Tensile per square inch.	Minimum Elongation in 8 inches.	Minimum Bending Cold, 1 inch diameter, or a rectangular section as near to this area as possible.	Percussive Test of the Article itself.
		Tons.			
1	Stems	26	10%	45°	Let fall bodily from a height of 12 ft. upon hard ground.
2	Stern-posts				
3	Rudder-frames	Do.	Do.	Do.	Do.
4	Tillers and side rods for steering	Do.	Do.	Do.	Do.
5	Paul plates and hoops for rudders.	Do.	Do.	Do.	Do.
6	Hawse pipes and deck pipes	Do.	Do.	Do.	Do.
7	Riding-bitts	Do.	Do.	Do.	Do.
8	Capstan	Do.	Do.	Do.	Do.
9	Cable compressor	Do.	Do.	Do.	Do.
10	Brackets for supporting after-ends of screw shafts.	Do.	Do.	Do.	Do.
11	Anchors	Do.	Do.	Do.	12 ft. fall on an iron slab, and to be tested 25% above the proof strain for iron anchors.
12	Wheels of various kinds	30	5%	20°	Fall from a height of 12 ft. on hard ground.
13	Rollers for turrets		Special test as at present to obtain crushing strain.		
14	Guard stanchions for bridges, &c.	26	10%	50°	To fall on an iron slab from a height of 10 ft.
15	Pump stanchions, and sockets	Do.	Do.	Do.	Do.
16	Racers clip, and other, for guns	Do.	Do.	Do.	The percussive test for clip racers as used in H.M. Service to be retained, or to be subjected to Hydraulic pressure at all points of its circumference.
17	Gun pivots	Do.	Do.	Do.	To fall upon an iron slab from a height of 10 ft.
18	Cylinders for mounting machine guns.	Do.	Do.	Do.	To fall on hard ground from a height of 12 ft.
19	Racers and clips to Medway mounting for Nordenfelt guns	Do.	Do.	Do.	To fall on hard ground from a height of 12 ft.

Appendix III.

No.	Article.	Description of Tests proposed to be applied.			
		Minimum Tensile per square inch,	Minimum Elongation in 8 inches,	Minimum Bending Cold, 1 inch diameter, or a rectangular section as near to this area as possible.	Percussive Test of the Article itself.
		Tons.			
20	Trollies for Gardner and Nordenfelt guns	26	10%	50°	To fall on hard ground from a height of 12 ft.
21	Tongs, water and bevil, for torpedoes.	Do.	Do.	Do.	Do.
22	Bed-plate for Nordenfelt guns .	Do.	Do.	Do.	Do.
23	Pivot-bars for guns . .	Do.	Do.	Do.	Do.
24	Deck-plate with double eyes and links for securing guns	Do.	Do.	Do.	Let fall on an iron slab from a height of 10 ft.
25	Clip for securing chains of guns	Do.	Do.	Do.	Do.
26	Drop bolts for guns . . .	Do.	Do.	Do.	Do.
27	Racks and pinions for turning turrets.	Do.	Do.	Do.	With the ends resting on hard ground, the racks should be let fall from an upright position. Pinion should be dropped from a height of 12 ft. upon hard ground.
28	Nippers for testing wire rope .	Do.	Do.	Do.	Let fall from a height of 10 ft. upon hard ground.
29	Frames for watertight doors .	Do.	Do.	Do.	One end rest on an iron slab, the frame raised upright, let fall from this position on to the slab.
30	Racks and pinions for sliding watertight doors.	Do.	Do.	Do.	Let fall upon hard ground from a height of 10 ft.
31	Cog wheels, &c., and rollers for watertight doors.	Do.	Do.	Do.	Do.
32	Clips and handles for watertight doors, manhole covers, scuttles, &c.	Do.	Do.	Do.	Do.
33	Hinges for watertight doors, scuttles, &c.	Do.	Do.	Do.	Do.
34	Sheaves, and cog wheels for steering gear.	Do.	Do.	Do.	Let fall from a height of 12 ft. upon hard ground.
35	Belaying cleats on plates, and belaying pins.	Do.	Do.	Do.	Let fall from a height of 10 ft. upon an iron slab.
36	Eye-plates for beams for compressor falls, &c.	Do.	Do.	Do.	Do.
37	Studs for lumber irons in iron beams.	Do.	Do.	Do.	Do.

Appendix III.

No.	Article.	Description of Tests proposed to be applied.			
		Minimum Tensile per square inch.	Minimum Elongation in 8 inches.	Minimum Bending Cold 1 inch diameter, or a rectangular section as near to this area as possible.	Percussive Test of the Article itself.
38	Awning stanchions . . .	26 Tons.	10 °/₀	50°	Let fall from a height of 10 ft. upon an iron slab.
39	Sockets for stanchions, awning &c.	Do.	Do.	Do.	Do.
40	Levers and sockets for opening side scuttles.	Do.	Do.	Do.	Do.
41	Collapsing screw for rigging, guard chains, &c.	Do.	Do.	Do.	Do.
42	Internal bindings for all descriptions of blocks.	Do.	Do.	Do.	Do.
43	Boats' crutches	Do.	Do.	Do.	Let fall from an height of 12 ft. upon hard ground.
44	Intercostal angles for longitudinals, beams, watertight work, &c.	Do.	Do.	Do.	Let fall from an height of 10 ft. upon an iron slab.
45	Blocks, large, for torpedo derricks, cat blocks, &c.	Do.	Do.	Do.	Do.
46	Solid ends for heads, and heels of pillars to beams.	Do.	Do.	Do.	Do.
47	Shoes for pillars to beams .	Do.	Do.	Do.	Do.
48	Tulips for ends of torpedo booms.	Do.	Do.	Do.	Do.
49	Sockets and eyes for ship side and eye hoops for torpedo booms.	Do.	Do.	Do.	Do.
50	Caps for masts and bowsprits .	Do.	Do.	Do.	Do.
51	Tulips and steps for masts .	Do.	Do.	Do.	12 ft. fall upon hard ground.
52	Stopper bolt for chain cables .	Do.	Do.	Do.	10 ft. fall upon an iron slab.
53	Brackets of various sizes and descriptions for stowing gear.	Do.	Do.	Do.	12 ft. fall upon hard ground.

As it may be necessary occasionally to repair some of the articles previously named it is desirable that all the steel used should be capable of being welded.

Memo.—The foregoing is not intended to be a complete list of the fittings which could be made in cast steel, but rather as a guide to the description of articles which could be made at once; this list could be added to after a trial of the articles named.

Some important articles, such as crank-shafts, propellers, framing for engines, &c., are now to some extent being made in cast steel, and the trials of these will probably greatly extend the use of cast steel in this direction.

DISCUSSION.

Mr. E. Reynolds: My Lord, I shall not detain you very long; I have in fact very little to say on the subject. What I have to say is on the subject of tests. Those proposed by the author are wisely directed solely to ascertaining that the material supplied is reliable without interfering with modes of manufacture. I only wish to make a suggestion as to what not to do, which is perhaps worth knowing. Within the last few weeks an advertisement has appeared in some of the scientific papers, emanating I believe from the Admiralty, with reference to anchors. In this advertisement it was proposed that three runners one inch square should be cast on each anchor which were to be used for tests. It would certainly not be practicable to get satisfactory or fairly representative tests in this way, but I have learned that in a particular case in which a number of articles were specified to be so tested, it was found very easy to get eminently satisfactory results by the simple process of laying pieces of bar steel in the moulds. It is, therefore, obviously desirable that this form of specification should be modified.

Mr. J. Riley: My Lord and Gentlemen, with regard to this matter of steel castings, it is one to which it is well known that we have devoted a considerable amount of attention, and I think have achieved a creditable amount of success. But there are points in regard to steel castings which I think it is desirable should be discussed here. Many of the failures which undoubtedly do occur in steel castings are due not to the manufacturer—not to the maker of the steel. The steel which is put into the mould is undoubtedly of the finest quality that can be made; but because of the want of experience or skill in those who have designed the articles which have to be cast, we are stranded. There is inequality of metal. The metal is not well disposed in the design. Brackets are put just in the places where they are most effective for destroying the castings; and when the casting is made, being bound by these brackets disposed as I have said, it cannot get room to contract, but is compelled to yield or tear somewhere in the process of cooling; and, forsooth, you are not competent to make a steel casting is the verdict immediately passed on you. Undoubtedly in the last three or four years many of the chief engineering firms in the country have given a great amount of thought to this matter of steel castings, and the proper distribution of the metal, having regard to what it is possible for a steel founder to make; and the result has been that the castings have been giving very much greater satisfaction of late years from that cause, added to the increased experience which steel founders have undoubtedly obtained themselves. But it is surely not intended that we should have tests proposed for steel castings such as those mentioned in connection with the anchors to which Mr. Reynolds referred, tests which Mr. Martell does not require should be borne in the case of ship plates. But it is the fact that we have been asked to give them, and in some cases engineers have tried to impose them upon us We have simply had to put our backs against the wall, and say, "It is impossible;" we cannot honestly give them, and we prefer rather not to undertake the work. I think it is not necessary to disclaim the putting of the bars into the box that Mr. Reynolds has referred to. Then there should be I think also some consideration as to the different tests required

for the different purposes for which the castings are intended. Mr. Warren in his able Paper divided them, and if those tests were uniformly adopted I do not think that any steel founder would complain, but that he would be able to give, all other things being equal, castings which would be perfectly satisfactory to those ordering them. I brought with me a draft of the test which we considered to be the correct tests for castings for different purposes, but those which Mr. Warren has given in his Paper are, I think, pretty much the same as I have here, and such as I think we should any of us accept.

Mr. B. Martell: My Lord, we are greatly indebted to Mr. Warren for the production of this paper, because this question of steel castings superseding iron forgings for many purposes in the construction of vessels is very important. If for many parts of a vessel we could get a solid homogeneous material like cast steel that would have sufficient malleability, it would be infinitely superior to many of the forgings which we are obliged to accept, and which we know, though we cannot see it externally, are very imperfect in the centre. I see that the tests proposed by Mr. Warren are very similar to those required by Lloyd's Register, and I think they are very proper tests. As Mr. Riley has said, I have just as great an objection to the tests required for these anchors, which were referred to by Mr. Reynolds and Mr. Riley, as they have. I think they are altogether superfluous. I have expressed that opinion before, but unfortunately it has made no impression. Instead of encouraging manufacturers in producing such splendid castings as some do make for many purposes—anchors particularly—it merely prevents manufacturers from going further in that direction. I quite agree in the remark that Mr. Warren has made, that there are "steel castings and steel castings." We have found from our experience that the manufacturers who, I do not say can, but who do, produce good solid satisfactory steel castings may be counted on the fingers of one hand. We have had castings which have been submitted to us, such for instance as quadrants for steering, and caps for masts, and purposes of that kind, to which if you applied a good strong hammer of moderate weight, you would smash them all to pieces like potters' ware. And some of these have been made by firms who rather occupy a high position in their own estimation, and as far as the extent of their works goes, you would really fancy they ought to be able to do something better. Our experience as to some of the castings which have been produced in that way has been such that we have positively refused to accept any more castings from them, no matter what tests they may be subjected to, until they produce some, and subject them to a test where we can test them to destruction in order to satisfy ourselves that they can produce satisfactory castings. Mr. Warren says it depends upon where you go to have your castings made. We know there are makers who can make them, though I should not like to go to the invidious length of mentioning any particular names. I see some of them in this room who can produce magnificent steel castings, and I should not hesitate to go even without any tests and give them an order to make them. There are others who are perfectly unreliable and from whom I would not accept castings under any circumstances, and that is how the question lies at the present time. I trust, however, that it will not be long before we are able to obtain, not from three or four makers, but from many, reliable steel castings which will supersede imperfectly welded forgings suitable for many parts in ship construction.

Mr. J. F. Hall: My Lord and Gentlemen, although the time is very short, I suppose the discussion upon this Paper cannot terminate without your expecting a few words from me. In the first place I would just like to refer to what Mr. Reynolds spoke of with regard to the proposed tests for anchors. Mr. Reynolds referred to that test as a test proposed by the Admiralty, but it was, in fact, proposed by the Board of Trade. I strongly object to these proposed tests, not because we cannot carry them out, but because the very cost of carrying them out would kill the whole trade of making anchors of steel castings. When you come to a costly article like a crank-shaft, or a large stern post or rudder, you have a chance of taking test pieces; but in the case of a small anchor—and some anchors are cast in three or four parts—to put three test pieces on each part, and prepare and test them besides carrying out the other tests imposed by the Board of Trade, it is simply impossible and absurd, and, if persisted in, will effectually destroy the manufacture of anchors in steel castings, which, I think, would be a great pity, particularly as we have got so far in producing anchors that stand very considerable strains, greater than any iron ones can. I must congratulate Mr. Warren, and, in fact, the Admiralty generally, upon the way in which they as a body have looked into the manufacture of steel castings. I am sure the way the Committee, when they visited our works, went into the thing and examined every detail, was in every way worthy of them; and I think also that the result of this paper, which is I notice to some extent taken from the Admiralty Blue Book on the subject, is one which does them very great credit. I only wish that all the surveyors we have to deal with went into the matter as thoroughly as they, and I will also add those of Lloyd's Register, have done. At the same time I cannot let this opportunity pass without making a few remarks on the very extraordinary result of this report to the Admiralty. I recognize on the first page, as to small castings, that most of the small articles shown in the Appendices were made by my firm. Several other firms made these small articles and with varying results. Now the very first time the Admiralty asked for any of these in practice was about six weeks ago, and what did they do? Why, they passed over all the firms who have given these tests, and ordered them from a firm who had never been visited by the Committee, and were utterly incapable of making small steel castings of a ductile nature. Of course, when I investigated the matter, I found it was entirely a question of price. This firm had no idea of what was required, and they quoted a very low price, and they made a very low article in consequence. For six weeks the Admiralty have been trying to get the castings from that firm and have failed, but last week they came to us and asked us to make them for them. (Laughter.) I think it is a pity, after all the work the Committee have done to get together all this evidence as to what steel castings should be, and what should be expected of them, that for the very first order they gave for one particular kind of castings that they took more trouble with than any other, except the large stern posts and rudders, they should have gone to a firm they never visited at all. I should just like to say a word with regard to the Nordenfelt castings. I know a good deal about the Nordenfelt castings, at least the so-called Nordenfelt castings, but if I mistake not, the process is that of an old friend of mine, Mr. Whittenström, of Stockholm, in Sweden. ("Hear, hear," from Mr. Riley.) Mr. Whittenström visited our Works some years ago, and I had an opportunity of discovering what he could do. He could make very small steel castings exceedingly well; but so could we, in fact. We could make them quite as well as Mr. Whittenström. I heard nothing more on the subject until recently, when these so-called Nordenfelt castings were so much talked about.

For the smaller class of castings, there is no doubt that this process is applicable, but up to now, I think they have had no experience, such as Mr Riley and myself have had, in making large castings for the stern posts and rudders similar to these which you see models of on the table. Each of these articles requires a certain consideration in applying the tests in regard to what that article has to do. For instance, this is a model of the engine frame of H.M.S. "Narcissus," built by Earle's Company, of Hull. You would not suppose that you required the same elongation and as low a tensile strain in a casting of that kind, which is to replace cast iron, and most of the strain of which comes, in compression, as you would on a rudder like this made for H.M.S. "Sans Pareil," and which has to undergo totally different strains altogether. Therefore, I think that in the matter of steel castings, as Mr. Warren has said, tests should be applied according to what the articles have to do. I could, of course, and should have liked to have said a great deal on this subject, but the time is now so short, my Lord, that I think I had better not say anything more.

Mr. W. H. White: My Lord and Gentlemen, as is well known the Admiralty includes a great many departments; I am connected with one, and although I was not connected with that department at the time this enquiry was set on foot, and which resulted in the Committee of which Mr. Warren was the chairman and Mr. John Barnaby one of the members, yet I now have to speak on behalf of the Admiralty. First of all I would like to say that the anchors which have been spoken of were not Admiralty anchors; the specifications were not Admiralty specifications, and the department in question was another department of the public service altogether. As regards Mr. Hall's complaint, it is a very open secret that whereas one department of the Admiralty settle what shall be the tests for any material required for shipbuilding, another department of the Admiralty makes the purchase. It is quite open of course to the public to say whether they think that is a good arrangement or not; but that is the fact, and when the conditions of a contract have been arranged, then the purchase proceeds in the manner I have described. But though I think the circumstance mentioned by Mr. Hall may have a good deal to do with Messrs. Jessop's business, it has very little to do with the use of steel castings, which is the subject we have been invited to discuss here. I quite agree with Mr. Reynolds that there is a broad distinction between the articles of which a sample can be tested to destruction, and the articles which you cannot test to destruction. I have heard of bars being laid in boxes—that is not news to me. When I was at Elswick we were amongst the first to go into large castings for ship work. We were extremely catholic in our enquiries; we did not mind where we got the castings, so long as they were good castings and fulfilled conditions of a reasonable nature, and I did there what I should always do under similar circumstances: I invited makers of steel castings to suggest tests. I did not accept always what they suggested, but that was the basis of the tests decided upon, and there was a wonderful agreement, I am bound to say, between the suggestions of firms who did not know that they were likely to be placed in competition. There must be variety in testing. In the Admiralty practice we have come very nearly to the recommendation Mr. Warren makes here, that is, that a minimum tensile strength is named with a certain guarantee of ductility. I would like to say one thing about steel castings of which I have seen a good deal, and that is that I fully believe in the maintenance of the falling test. I have seen strange things happen to castings which were made by firms of eminence, and in the preparation of which I believe the utmost care had been taken,

and I think that nothing short of a falling test would have discovered such defects; and I am sure that the makers of the castings will agree with me that this is a test which had better be retained. I am not saying that every article should be made to fall in a particular way. There are some things which could be let fall in such a way that they would almost inevitably break, but I do say that we want to bring shock and concussion upon this material, or we may subsequently make discoveries which it would have been better to have made at an earlier time. If there is any fault in the steel it should be found out before it is used. As regards Mr. Martell's remarks (he is not present now), I presume in this matter of steel castings we are passing through a phase very much like what we passed through with mild steel. There are a few of the leading firms in the country who have devoted themselves to the development of this manufacture. In my judgment the shipbuilding and engineering professions owe to these firms the greatest thanks for having enabled us to do what is most valuable, and that is to avoid the delays which so often arose when heavy forgings had to be made for important parts of the structures of ships and engines. To those who have had to conduct shipbuilding operations or engineering works, it is an enormous relief to be almost certain that you can get in a short time a casting which will take the place of a forging, be thoroughly trustworthy, and answer your purpose in every way, besides having the modest advantage of being cheaper.

Mr. A. E. Seaton: My Lord, having used steel castings very extensively for engines and in shipbuilding for the last eight years, and in that time having had pass through my hands many hundreds of tons of that material, I have had opportunities of forming some judgment on it. My chief excuse, however, for intruding now is to call attention to a point which I think has been a little overlooked; especially would I wish to press it on the minds of steel manufacturers present. There are two objects which we have in view in using steel castings; one the substitution of them for expensive and difficult forgings, the shapes of which necessitates a considerable amount of machine work, the other the substitution of a steel casting for a brass casting on the score of expense, or for an iron casting in order to get the additional strength due to steel and avoiding the liability to fracture from sudden cooling. For these two purposes we can do with two different kinds of material. In the one case we want a steel casting having the same high tensile strength, with the same toughness as an iron or steel forging, in the other case we can do with a material of very much lower tensile strength, and with considerably less toughness. Now when I first began to use steel castings, the Sheffield manufacturers, notably some of the firms represented here to-day, were enabled to supply them; but for the purpose I wanted them the price was a little prohibitive, and they had one fatal defect, that was that on machining I did not get the soundness that was necessary to convince superintendent engineers and others that it was a fair substitute for wrought iron, for now a little honeycombing is deemed by some a very damning defect in steel castings when machined. I do not share that prejudice myself, because experience has taught me otherwise. I found I was able to get from Germany castings quite free from defects of that kind, so that, when machined, they almost invariably gave a perfectly clean surface and answered my purpose admirably. I admit that the German steel castings are by no means so strong, taking them generally, as the English, and the tests I made with them showed that the elongation at fracture was no more than 6 or 7 per cent., but it was far superior in strength to cast iron, and superior even to the ordinary forged iron and gun metal castings, therefore it answered the purpose for which

I required it. There was one other point I may mention. These German castings were at that time only about half the price of the best English makers. Castings that our best English firms asked 45s. or 50s. per cwt. for, I could get for between 25s. to 30s. in Germany. I must say I sent orders to Germany with very great reluctance, and I never got a batch of castings but I invited the representatives of the Sheffield houses to see them, to be quite convinced that they were what I represented them to be, and I strongly advised them to learn to make them as soon as possible, at least as sound if they could not do them as cheaply. I said then what I am prepared to say now, that I would sooner give a little higher price for sound English castings than for sound German ones, but I was not prepared to give any price for an unsound English one when I could get a sound German one. There was something I noticed in one of these particular castings worthy of notice. A very large eccentric strap, after it was bored out, displayed a very fine crack shown to answer to what I may call the longitudinal dimensions of the strap. It was a very fine hair line that could only be seen well with a magnifying glass. We put it under a steam hammer, and tried to break it through this crack, but although we eventually broke the strap into small pieces, we could not get it to break through the crack. Some question has been raised this morning as to percussion, or perhaps it would be more correct to say concussion, to which these castings may be subject. Some two years ago we supplied some steamers with cast steel connecting rods. It was a suggestion of my own, and intended as a step to carry out my own idea of generally substituting soft steel castings for expensive forgings. I had the permission of the owners to fit these connecting rods subject to certain tests. One of the tests we adopted, at the suggestion of the engineers who had said: "You may get a very good test from the material; everything may be very good and proper; the steel may be tough and strong, but how is it going to be after the ship has run so many thousands of miles, and each connecting rod has had so many blows on end?" In order to see how far percussion did affect the nature of the steel, we cut out of the eye at the rod end a solid lump, and cut it up into strips. We took one piece and turned it down and tested it, and noted the elongation, the ultimate strength and reduction of area. The second piece was put under a tilt hammer—I have forgotten the weight of it now, but it does not signify very much—and was subjected to five thousand blows. It was turned down and tested, and showed no material alteration in the strength or other physical qualities of the steel. Then we subjected a third piece to ten thousand blows, and although the material was really what blacksmiths call upset, and was seriously damaged at the ends from the blows, still the turned down part in the middle was only slightly affected. It undoubtedly was affected, but to no very serious extent. One or two of these models here to-day I think I recognize as being models of castings used by us for engine framing, &c., and some of stern frames of our ships. I do think that the tests insisted on both by the Admiralty and the Board of Trade are very severe for steel castings, and I think so, because no iron forging with welds in it would for an instant withstand the heavy fall to which a big steel casting is subjected in testing, and considering that Lloyd's do not allow us to make any reduction in scantling for these stern frames and stems, they should not insist on any more testing for cast steel than for the forging. If we had claimed a reduction of scantling, then I say Lloyd's have the right to ask us to prove that the material warrants such a reduction; but since they do not grant that reduction I do not think they should put these forgings to such a severe test. But notwithstanding all these severe restrictions, it has been a great feather in the cap of steel makers, that these steel castings have come

through really so successfully. I think it quite warrants us in proceeding as we are doing in the substitution of cast steel for forgings both in ships and engines. This feature is an idea of my own to substitute steel castings for the combination of struts and rods connecting the main bearings and foundation of the engine to the cylinders. It is a very difficult casting to make, but my friend Mr. Hall has succeeded in giving us some very good ones indeed. I hope they will prove what I anticipate they will, and give both a strong and a stiff engine frame.

Mr. E. C. Warren: My Lord, I am glad to hear the general approbation which has been expressed with regard to the Paper. I think if Mr. Seaton will, at his leisure, look carefully into the tests proposed here he will find that such objectionable tests as he refers to are not included in the proposals here made. I may say that these tests generally have been approved by the Admiralty, and that demands are generally made now on the lines of the tests here given, and when I say that these tests have not only received the sanction of the Admiralty, but also of a large number of the principal steel firms in the country, who have also agreed to them, I do not think Mr. Seaton need be under any apprehension about the tests proposed. I admit that some of the tests to which he refers, and of which Mr. Riley knows something, were absurd; they were evidently proposed by some one who knew very little about steel castings, and were very properly denounced by cast steel manufacturers. I am sorry so little time could be given to the reading and discussion of this Paper owing to the unusual time occupied by the reading and discussion on the two previous Papers. I will not say more at this late hour, than to thank you, my Lord, and the meeting for the attention and consideration given to this Paper.

The President: Gentlemen, I am sure you will allow me to thank Mr. Warren for his very able Paper. I regret very much, and I am sure that many of you will regret, that unfortunately the time at our disposal has necessarily very much curtailed the discussion upon his useful Paper. Now, gentlemen, I want to announce to you that Sir Nathaniel Barnaby has most kindly acceded to a request I made to him to take the chair this evening. I am sure you cannot possibly have a more efficient chairman. I wish he was always here. I also must remind you that on Saturday morning the Council have agreed, in consequence of the number of Papers which it was impossible to cram into the usual three days, there is to be "a morning sitting," to use a parliamentary phrase, for the reading of two Papers for which we could not possibly otherwise have found room. But we wish to show our appreciation of the labours of those who contribute Papers, and therefore we have made that extension of time for the purpose of letting in those two Papers.

CLOSED STOKEHOLDS.

By Richard Sennett, Esq., R.N., Member of Council.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 15th, 1886 ; Sir Nathaniel Barnaby, K.C.B., Vice-President, in the Chair.]

The application of forced draught to the furnaces of marine boilers is a subject of considerable interest at the present time, and has, in one form or other, been brought before the notice of this Institution at each of our last three annual meetings.

The only system that has yet had any extended practical trial is that of closed stokeholds worked under air pressure, which was described by Mr. R. J. Butler at the meetings in 1883, when he gave particulars of the application of the system to H.M.S. "Conqueror" and "Satellite," and the results of the trials made to ascertain its efficiency. These trials, though incomplete in some respects, as explained by Mr. Butler, were of great value for guidance in future designs, and showed clearly that the generative powers of the boilers could, by a simple and comparatively inexpensive method, be very materially increased.

Since that time all the ships built for the Royal Navy, and many other vessels, have been fitted with this system, and the results obtained have been most satisfactory. My object in writing this paper has been to lay before the members of this Institution the results obtained during some of the more recent trials. As to the value of these results, and the promise they show of future progress, you will be able to form your own judgments.

Detailed particulars of the machinery of the ships, and the results of their trials, are given in the Tables attached, and it is only proposed in the body of the paper to deal with the leading features of the subject.

All the trials were made in accordance with the usual Admiralty practice. The indicator diagrams were taken at regular half-hourly intervals, and the revolutions were taken, at the end of each half-hour, from the indications of mechanical counters worked off the engines, the mean of the half-hourly totals being used for the calculation of the

I.H.P. The results may, therefore, be safely relied on as the average performances during the trials.

It will be unnecessary to occupy time in describing the method of application of the system of closed stokeholds to the ships in question, as it is practically the same as that explained by Mr. Butler in his paper, simplified and improved in some details as experience has been gained.

We will first take the case of the twin-screw armour-clad barbette ship "Rodney." She is fitted with 12 boilers, arranged in four separate and independent stokeholds, the working steam pressure being 90 pounds per square inch. The arrangement of the fans and screens in each stokehold, for forced draught working, is shown in Figs. 1 and 2, Plate XX. There are two fans, 5 feet in diameter, for each stokehold, one at either end, fixed on the debris deck that forms the air-tight ceiling of the stokehold. The engines of the "Rodney" are vertical 3-cylinder compound, each set having one high and two low pressure cylinders, the ratio of the volume of the high to that of the two low pressure cylinders being 1 to 4·05. The proportions of the boilers and engines are such, that with natural draught alone, an I.H.P. of from 7,500 to 8,000 would be developed.

A four hours' trial with forced draught was made on the 13th June, 1885, all 12 boilers being used. The mean I.H.P. developed was 11,158, with an average air pressure in the stokeholds equal to 1·4 inches of water. This gives a mean of 14·75 I.H.P. per square foot of fire-grate, and of 18·5 I.H.P. per ton of boiler (including water, uptakes, fittings, &c.).

It was seen during this trial that the boiler power was in excess of the capacity of the engines to utilize the steam generated, and a subsequent trial was, therefore, made on the 16th June, 1885, with nine boilers only in use. The air-pressure during this trial was gradually raised from $1\frac{1}{2}$ to 2 inches of water, and the average I.H.P. developed was 9,544, which is equal to 16·83 I.H.P. per square foot of fire-grate. During the last hour of this trial, when the air pressure was kept equal to two inches of water, the mean I.H.P. developed was 9,760, or 17·2 I.H.P. per square foot of fire-grate. The boilers generated an ample quantity of steam, the safety valves blowing freely throughout the trial.

The forced draught trial of the "Howe," a sister ship to the "Rodney," was made on the 14th January, 1886; the maximum I.H.P. developed was 12,118, and the average for the four hours 11,725, or at the rate of 15·5 I.H.P. per square foot of fire-grate.

We will next take the case of the fast twin-screw cruiser "Mersey," whose official forced draught steam trial was made off the Nore on the 24th September, 1885. The "Mersey" is fitted with six boilers of the low cylindrical marine type, arranged in two stokeholds; and with natural draught alone would be equal to about 4,000 I.H.P. The working steam pressure is 110 pounds per square inch. The general features of the stokehold arrangements are shown in Figs. 3 and 4, Plate XXI. In the "Mersey" the supply to the fans is through air trunks carried direct to the upper deck. Two 5-feet fans are fitted for each stokehold. The engines of the "Mersey" are horizontal compound, each set having one high and one low pressure cylinder, the ratio of the high to the low being 1 to 2·81. The average I.H P. developed during the trial was 6,628, with an air pressure in the stokeholds equal to 2 inches of water. This represents 16·61 I.H.P. per square foot of fire-grate, and 21·7 I.H.P. per ton of boiler (including water, uptakes, fittings, &c.). This is the highest result that has yet been obtained with ordinary marine boilers.

The results of trial of the torpedo cruiser "Scout" were practically the same as those of the "Mersey." The machinery and boilers are of similar type but smaller. The "Scout" has four boilers, two in each stokehold, and the working pressure is 120 pounds per square inch. The average I.H.P. developed on the forced draught trial made at Spithead, on the 23rd September, 1885, was 3,370, which is equal to 16·28 I.H.P. per square foot of fire-grate, and 19·3 I.H.P. per ton of boiler.

In the development of this system of forced draught, care has been exercised to ensure that each step taken was safe, and justified by previous experience, in order to avoid the risk of failure from taking a too sanguine view of the possibilities of the system. As a matter of fact, in no case yet, on a full power trial, have the boilers been worked to the full extent of their capabilities, and the reserve of power in the fans has kept the steam supply always fully under command.

In some trials made by using a portion only of the boilers, so that all the steam generated could be readily utilized by the engines, the results obtained have been higher than those quoted from the full-power trials. Several trials of the machinery of the sloop "Caroline" were made in basin at Sheerness in March, 1885, with only one-half of the boilers in use. The results of one of these trials, of six hours' duration, are given in Table attached, from which it will be seen that, with an air pressure equal to not more than $1\frac{1}{2}$ inches of water, 18 I.H.P. was developed per square foot of fire-grate. The "Caroline" is a sister ship to the "Satellite," referred to in Mr. Butler's Paper, and the particulars and arrangements of the machinery and boilers are practically the same as in that ship.

To enable the comparative value of these results to be properly appreciated, I have shown in the following Table particulars of the performances of similar ships, some with ordinary open stokeholds, and others with closed stokeholds and forced draught :—

	1	2	3	4	5	6	7	8
	Ship.	Date.	Load on safety valves.	I.H.P.	Weight of boilers.	Area of fire-grate.	I.H.P. per sq. foot of fire-grate.	I.H.P. per ton of boiler.
			lbs.		Tons.			
Open Stokeholds.	Inflexible . .	1878	60	8,483	756	829	10·21	11·22
	Colossus . . .	1883	64	7,492	594	645	11·62	12·61
	Phaeton . . .	1884	90	5,588	462	546	10·23	12·1
Forced draught.	Howe . . .	1885	90	11,725	632	756	15·54	18·5
	Rodney (9 boilers) .	1885	90	9,544	474	567	16·83	20·1
	Mersey . . .	1885	110	6,628	306	399	16·61	21·7
	Scout . . .	1885	120	3,370	174	207	16·28	19·3
	Trafalgar (estimated)	—	135	12,000	514	609	20·00	23·3

NOTE.—The weight of boiler given includes weight of water, funnel, uptakes, fittings, spare gear, &c.

By referring to the seventh column of this Table, it will be seen that whilst in the ships with natural draught only, about $10\frac{1}{4}$ I.H.P. was developed per square foot of fire-grate, between 16 and 17 I.H.P. was obtained with moderate forced draught, the boilers being practically the same in the two cases. The steam blast was used throughout the trial of "Colossus."

The grate area can only be used as a fair basis of comparison for boilers similar in design and construction. I have, therefore, in the last column, given the I.H.P. developed per ton weight of boiler, which is the more important feature, so far as the naval architect is concerned, and we see from this column that the effect of the application of forced draught has been to increase the power obtained from a given weight of boilers in the proportion roughly of 20 to 12, the engines and boilers being of practically the same description in both cases.

In the "Nile" and "Trafalgar," and other warships now building, triple expansion engines will be fitted, worked with steam of 130 to 140 lbs. pressure per square inch.

From the experience we have now gained respecting the steam generating powers of boilers in closed stokeholds kept under moderate air pressure, and the well-known economical employment of steam in triple expansion engines, we are satisfied that on the full power trials of these vessels, at least 20 I.H.P. per square foot of grate, and between 23 and 24 I.H.P. per ton of boiler will be realized, and this condition has been readily accepted by the engine contractors, who have had experience of the working of the system.

As before stated these results are obtained without any undue forcing of the boilers, the maximum limit of the air pressure allowed on the Admiralty trials being equal to two inches of water, or about $\frac{1}{13}$th of a pound per square inch, and the boilers have in no way suffered from the work. It is probable that, as further experience in the working of the system is gained, the air pressure in the stokeholds may be safely maintained at a somewhat higher point than two inches of water, with a corresponding increase in the steaming powers of the boilers.

The many advantages as regards the power and speed of steamships, that are gained by this great reduction in the weight and space required for the boilers are too obvious to require enumeration, and it is perhaps not too much to say that the very high speeds that have been recently obtained by several cruisers of moderate dimensions would have been impracticable without the application of forced draught.

There are several subsidiary advantages resulting from this system of working, which hould be briefly mentioned.

The ventilating pipes with large reversible cowl-heads above the upper deck, necessary for natural ventilation, but which are generally objectionable, and frequently interfere with the working of the ship or guns, can be to a great extent, if not altogether, dispensed with and the openings in the deck reduced to the minimum required for supply of air to the fans, and for access to the engine and boiler rooms, &c. In the larger ships, as shown in Figs. 1 and 2, the fans are simply fixed on the debris deck above the boilers, and the air finds its way to them from the ordinary ship's hatchways, no special ventilating trunks being necessary.

It has always been requisite in large warships, in consequence of the difficulty of arranging suitable openings in the upper deck and carrying air trunks from them to the stokeholds for natural ventilation, to fit fans to blow air into the stokeholds, and the system of closing in the stokeholds, now adopted, is only the completion of the original plan, in order to increase the supply, and to ensure that the air blown into the stokeholds shall pass through the fires.

When blowing fans are fitted to supply the air for the fires in closed stokeholds, the power of the ship is practically independent of the force and direction of the wind, which is a matter of no small importance, especially when steaming in the Tropics. By the use of the fans the fires may be kept working uniformly, and the exhausting work of raking and stirring the fires, so frequently required during calm weather with open, natural draught stokeholds, is avoided, and the power developed can be easily and quickly regulated by the speed at which the fans are driven.

Stokeholds, worked under air pressure, are also necessarily kept cool and well ventilated under all conditions, which is beneficial to the stokers, and is a point that does not appertain to any other system of forced draught. When the system was first adopted it was suggested that the men might have some reluctance to work in closed stokeholds. This, however, has not proved to be the case, and the men have, from the first, worked as confidently and cheerfully as in ordinary stokeholds. The only effect that the closing of the stokeholds has had upon the men has been to enable them to do their work in more comfort, in consequence of the better ventilation.

It is unnecessary to occupy time in enumerating other minor points of advantage possessed by the closed stokehold system, which will occur readily to all who have had experience in its working.

There is, however, one important feature about which a wrong impression appears to exist in some quarters. It is assumed that the system is necessarily uneconomical, so far as the consumption of coal is concerned. This, however, is not the case; the closed stokehold system, in common with other good forced draught systems, tends to promote economy of fuel, in consequence of the better supply of air and the higher temperatures at which the fires are worked. It is true that on some full power forced draught trials of ships in the Royal Navy, the consumption of coal per I.H.P. has been somewhat in excess of the expenditure required for natural draught alone. This, however, is not due to the method in which the coal is burnt, but simply results from the waste heat that passes up the funnel in consequence of the comparatively small proportion of heating surface to the coal burnt.

Engines of warships are only occasionally required to work at their maximum power, and it is considered preferable to accept the slight loss that occurs on these occasions in consequence of the waste heat in the escaping gases, rather than to permanently increase the weight of, and space occupied by the boilers, by providing heating surface fully equivalent to the maximum rate of combustion of coal in the furnaces. For all ordinary

cruising the closed stokehold system is most economical; the furnaces are supplied with the proper quantity of air they require, and any sudden increase of power necessary is readily obtained by simply driving the fans faster.

So far as warships are concerned, I have no doubt whatever that, at present, it is the only system that commends itself for general application, and I look forward with confidence to considerable extension in its application and usefulness. It has been adopted not only for ships for the Royal Navy, but also very largely by the designers of warships for foreign navies.

How far it is applicable to mercantile steamers making long sea voyages it is, perhaps, scarcely within my province to say. It has been fitted successfully by Messrs. Laird to the Dublin and Holyhead Royal Mail Packets, and I have no doubt that very great advantage would result from its adoption for other steamers employed on similar service.

There is also every reason to believe that the system of closing the stokeholds and fitting blowing fans might be applied to ocean steamers generally with much advantage. The closing of the stokehold is quite independent of forcing the draught. The fans could be made to supply the furnaces with air, without producing any appreciable pressure in the stokeholds; or, if pressure be used, the area of the heating surface of the boilers could be designed to suit the increased rate of combustion.

In large ocean steamers the necessary air trunks and casings, carried above the upper deck, to supply the air required for the stokeholds, occupy valuable space, and the power and speed of the ship, especially on some services, must depend largely on the force and direction of the wind. If the stokeholds were closed in, and the air to them supplied by fans placed on the deck, in a somewhat similar manner to that adopted in warships, the state of the weather would be of small importance, so far as the steam-generating powers of the boilers were concerned, and the power and speed of the ship could be maintained under all conditions. This would doubtless tend to reduction in the space and weight required for the boilers, and lead to improvements in many other ways. I would not presume to dogmatise on this point, but merely throw out the suggestion for the consideration of those members who are more intimately concerned with the design of machinery for the mercantile marine.

TABLE I.

Particulars of Machinery.

Particulars.	*Inflexible.*	*Colossus.*	*Phaeton.*	*Mersey.*	*Scout.*	*Rodney* and *Howe.*	*Trafalgar* proposed.
Description of Engines	3 cylinder vertical compound.	3 cylinder vertical compound	Horizontal compound	Horizontal compound	Horizontal compound	3 cylinder vertical compound	Vertical triple expansion.
Diameters of cylinders in ins. — High pressure	2 of 70″	2 of 58″	2 of 42″	2 of 38″	2 of 26″	2 of 52″	2 of 43″
Diameters of cylinders in ins. — Low pressure	4 of 90″	4 of 74″	2 of 78″	2 of 64″	2 of 46″	4 of 74″	2 intermediate of 62″ 2 of 96″
Length of stroke ft. ins	4′ 0″	3′ 3″	4′ 0″	3′ 3″	2′ 6″	3′ 9″	4′ 3″
Propeller — Description ...	2 bladed	4 bladed	4 bladed	3 bladed	3 bladed	4 bladed	not yet decided.
Propeller — Diameter ft. ins.	20′ 2½″	17′ 8½″	14′ 0½″	13′ 0″	10′ 6″	15′ 6″	
Propeller — Pitch ft. ins.	23′ 0½″	18′ 7¼″	20′ 1¾″	18′ 5⅞″	12′ 6″	19′ 6″	
Boilers — Number	12	10	8	6	4	12	6
Boilers — Description	Four each of: Oval 3 furnace; Oval 2 furnace; Oval Double ended 4 furnace	Eight Oval 3 furnace; Two Oval 2 furnace	Cylindrical high 3 furnace	Low Cylindrical 3 furnace	Low Cylindrical 3 furnace	Oval 3 furnace	High cylindrical 4 furnace
Boilers — Transverse dimensions	13′7″ × 15′6″ / 11′1″ × 13′4″ / 9′4″ × 14′3″	12′9″ × 15′3″ / 7′10″ × 14′0″	13′ 5″ dia.	10′ 0″ dia.	9′ 3″ dia.	11′0″ × 15′0″	16′ 2″ dia.
Boilers — Length ft. ins.	9′ 0″ / 9′ 0′ / 17′ 0″	9′ 9″ / 9′ 9″	9′ 8″	18′ 9″	17′ 10″	9′ 8″	10′ 3″
Load on safety valves lbs.	60	64	90	110	120	90	135
Furnaces — Number	36	28	24	18	12	36	24
Furnaces — Diameter	twelve of 3′ 6″ / eight of 3′ 3″ / sixteen of 3′ 6″	Twenty-four of 3′ 5″ / Four of 2′ 10″	18 of 3′ 3″ 6 of 3′ 0″	3′ 2″	2′ 10″	3′ 0″	3′ 7½″
Furnaces — Length	6′ 0″ / 6′ 0″ / 6′ 6″	6′ 9″ / 6′ 9″	7′ 0″	7′ 0″	6′ 0″	7′ 0″	7′ 4″
Furnaces — Grate area in sq. ft.	829	645	546	399	207	756	609
Heating surface of boilers in sq. ft. — Tubes	18,654	14,747	12,456	10,367	5,500	17,174	17,040
Heating surface of boilers in sq. ft. — Total	22,288	17,507	14,562	11,700	6,170	20,294	19,390
Area through tubes in sq. ft. ..	158	117	87·5	61	32	102	96
Funnels — Number	2	1	2	1	1	2	2
Funnels — Size	Oval 10′ 0″ × 8′ 0″	Oval 12′ 0″ × 8′ 0″	8′ 0″ dia.	7′ 2″ dia.	6′ 6″ × 4′ 9″	9′ 0″ × 5′ 6″	7′ 0″ dia.
Funnels — Height above fire bars	70′ 3″	80′	61′ 8″	52′ 6″	55′ 0″	75′ 0″	65′ 0″
Ratios of — Tube heating surface / Grate area	22·5	22·8	23·3	25·9	26·5	22·7	28
Ratios of — Area through tubes / Grate area	·190	·181	·160	·152	·154	·134	·158
Ratios of — Area of funnels / Grate area	·160	·128	·183	·100	·125	·114	·126
Forced draught fans — Number ...	—	—	—	4	4	8	6
Forced draught fans — Diameter ft. ins.	—	—	—	5′ 0″	3′ 6″	5′ 0″	5′ 6″

TABLE II.

Abstract of Steam Trials.

	OPEN STOKEHOLDS.			FORCED DRAUGHT.					
	Inflexible.	*Colossus.*	*Phaeton.*	*Mersey.*	*Scout.*	*Rodney.*		*Howe.*	*Caroline.*
Date of trial	14th Nov., 1878.	10th Jan., 1884.	12th Feb., 1884.	24th Sept., 1885.	23rd Sept., 1885.	13th June, 1885.	16th June, 1885.	14th Jan., 1886.	4th March, 1885.
Duration of trial in hours	6	5	5	3	4	4	3	4	6
Number of boilers used	12	10	8	6	4	12	9	12	2
Mean steam pressure in boilers, lbs.	61·06	61·52	85·35	107·8	113·09	93·06	92·74	89·21	84·52
Mean air pressure in boiler rooms, ins. of water	...	...	...	2·02	1·52	1·4	1·89	2·05	1·5
Mean pressure in cylinders in lbs., per square inch — High pressure	29·55	40·66	43·56	56·53	61·42	59·92	49·73	59·51	43·9
Mean pressure in cylinders in lbs., per square inch — Low pressure	9·833	12·09	11·43	22·82	24·31	12·8	12·1	13·43	12·79
Mean revolutions per minute	73·26	89·96	100·26	122·34	152·33	103·42	100·13	106·63	77·8
Mean speed of piston, in feet, per minute	586	585	802	795	762	776	751	800	389
Indicated horse power	8,483	7,492	5,588	6,628	3,370	11,158	9,544	11,725	983
Area of fire grate used in square feet	829	645	546	399	207	756	567	756	54·5
I.H.P. per square foot of fire grate	10·21	11·62	10·23	16·61	16·28	14·75	16·83	15·51	18·02
Heating surface per I.H.P. in square feet — Tubes	2·20	1·97	2·23	1·56	1·63	1·54	1·35	1·46	1·24
Heating surface per I.H.P. in square feet — Total	2·63	2·33	2·61	1·77	1·83	1·82	1·6	1·73	1·43
Coal used per I.H.P. per hour, in lbs.	2·06	2·55	2·39	2·48	2·6	2·2	...	2·16	2·54
Coal used per hour, in tons	7·80	8·53	5·96	7·33	3·92	11	...	11·30	1·11
Remarks	Blast used last ½ hour only.	Blast used throughout the trial.	Natural draught only.						

Note—The indicated H.P. recorded, is that developed by the main engines only, and does not include the I.H.P. expended in working the feed and circulating pumps, blowing fans and other auxiliary machinery.

ON FORCED COMBUSTION IN FURNACES OF STEAM BOILERS.

By James Howden, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 15th, 1886; Sir Nathaniel Barnaby, K.C.B., Vice-President, in the Chair.]

In a Paper I had the honour of reading at our Annual Meetings of 1884, "On Combustion of Fuel in Furnaces of Steam Boilers by Natural Draught and by Supply of Air under Pressure," I directed attention to the following facts :—

1. That the great reduction in the consumption of fuel, effected during the last 25 years, in the production of steam power had not been in any way due to improvement in the evaporative economy of the boiler, which, on the contrary, was now greatly less than was common in Cornwall 40 years ago, in boilers carefully worked with slow combustion.

2. That this retrograde movement was the inevitable result of the harder working of boilers in our day, necessitated by increasing limitations of time and space, without the adoption of compensatory means to meet the changed conditions.

I likewise pointed out that while the demand on the productive power of boilers was increasing, this demand could not be accomplished by natural draught without a still further reduction of economy, and that the resort to forced draught for increase of power had hitherto resulted in further aggravating this loss of economy, and increasing the wear and tear of boilers in a greater ratio than that of the power produced.

I further described a system of forced combustion I had devised with a view to meet the demand for increased power in reduced space, and at the same time to reverse the conditions on which such increased power had hitherto been attained, viz. :—increased consumption of fuel, and wear and tear of boilers,—and give, instead, this increased power with less wear and tear of boilers, and an economy approaching, if not equalling, that of the now almost historical high Cornish practice.

This somewhat ambitious endeavour I was led to make some few years ago, on what appeared to me a sound theoretical basis, guided practically by the experience of a similar

attempt, instructive though unsuccessful, made many years before. The soundness of my theoretical basis I had practically confirmed by trials, extending from 1882 to 1884, made chiefly in a multitubular boiler, 10 ft. in diameter by 9 ft. in length, having two furnaces, each 3 ft. diameter, which I had built and erected for the purpose of testing it. The results obtained from this boiler gave me what I considered sufficient reasons for claiming the advantages for this system of combustion stated in my previous paper.

It will probably be in the recollection of some present that the remarkable economy and other advantages which I then claimed for my system of combustion, from what I had then accomplished, were considered of a somewhat sanguine character, and not likely to be realized in ordinary practice.

Not being able, at the time, to produce confirmation of the results developed by the trials of my experimental boiler, by any example of its working in a sea-going steamer, I could only then affirm the correctness of the statements made, and promised to lay before the Institution further information when obtained.

In fulfilment of this undertaking I have now the satisfaction of bringing before the Institution additional facts, amply proved by working at sea over a lengthened period, which more than corroborate my former statements, and fulfil my anticipations.

The case which chiefly supplies the facts stated in this Paper is that of the Steamship "New York City," of the "Direct" West India Line, of Messrs. Scrutton, Sons and Co. I am indebted to Mr. Thomas Scrutton of this firm for his appreciation of the value of what I had then accomplished, and for having—undeterred by the halting opinions of those who are considered guides in such matters—placed an order in the hands of my firm, shortly after the date of the reading of my Paper in 1884, for the refitting of the before-mentioned steamer with a boiler to be worked on my system of forced combustion.

The degree of confidence placed by Mr. Scrutton in my statements will be better understood when the nature of the voyages to be performed, and the size of boiler I proposed, are considered. The voyage is from London direct to the West India Islands, and through the Archipelago there, extending at intervals to Demerara and Honduras, the round voyage occupying, according to circumstances, from 3 to $4\frac{1}{2}$ months. The opportunities on the voyage for obtaining fresh water for the boiler are so limited that, as a rule, salt water only is used.

The boiler I proposed, to replace the one in use, was to be considerably less in size and in furnace and heating surface measurements, and therefore insufficient for natural

draught working. As the route in which the steamer makes her long voyages is not favoured with the repairing shops to be found on most other routes, failure or injury to the boiler would have meant disaster.

The "New York City" is a steamer built in 1879 on full load lines for large dead-weight carrying, her dimensions being 260 ft. × 34½ ft. × 22½ ft., gross tonnage 1,724, and displacement, on full load draught of 20 ft., 3,700 tons nearly.

The engines are ordinary compounds, having cylinders 33 in. and 61 in. diameter by 33 in. stroke, and without an expansion valve on h.p. cylinder. The original boiler, though worked as one double ended boiler with two furnaces at each end, consisted of two separate cylindrical parts, each 12 ft. 6 in. diameter by 6 ft. 4½ in. in length with large steam domes. These cylindrical parts were tied together at the distance apart of 4 ft. 3 in., this space, forming a dry combustion chamber, being closed in with an iron casing lined with fire brick.

Messrs. Scrutton inform me that this boiler, when properly worked, compared fairly as regards economy with those of their other steamers, but was subject to leakage in the tube plates of the combustion chamber, when not carefully preserved from sudden admission of cold air.

The new boiler, Figs. 1 and 2, Plate XXII., supplied to the steamer to work with forced combustion, is single ended with three furnaces, and occupies so much less space in the ship that, after providing for an unusual roomy stokehold, sufficient space for 120 tons additional coal could have been got in former boiler room.

The following are the principal particulars of the two boilers :—

	Original Boiler.	New Boiler.
Length without uptake	17 ft. 0 in.	11 ft. 0 in.
Diameter	12 ft. 6 in.	14 ft. 0 in.
Steam domes	two	none
Number and diameter of furnaces .	four 3 ft. 5 in.	three 3 ft. 4 in.
Number, length, and diameter of tubes.	372 – 6ft. 4½ in. × 3½ in.	210 – 8 ft. 0 in. × 3 in.
Tube surface	2,173 sq. ft.	1,319 sq. ft.
Length of fire bars, over all . . .	5 ft. 6 in.	4 ft. 1½ in.
Aggregate fire grate	75 sq. ft.	36 sq. ft.

The diameter of the new boiler is somewhat greater than is required for the size of furnaces or number of tubes. The diameter of the tubes also is larger than would have been used had I not undertaken in the contract to develop a certain power with natural draught. The size is therefore a compromise between chimney and forced draught proportions. The air-heating tubes for each furnace are 40 in number, 2 ft. 3 ins. in length, and $3\frac{1}{4}$ ins. external diameter, they also being proportioned, so far, in view of the boiler being worked with natural draught.

The air space in the fire grates was reduced in width by side pieces to rather less than 2 ft. 11 ins. across, so that each fire grate was, with 4 ft. $1\frac{1}{2}$ ins. length of bars, 12 square feet in area. A twofold object was sought for in using these side pieces, one being to prevent the too rapid combustion of the fuel at the sides of the furnaces, and the other to reduce the heat somewhat, at a part where the plating is unfavourably placed for throwing off the water evaporated, being below the centre line of the furnace.

The boiler was fitted on board in September, and, after the loading of the steamer for outward voyage, was tried for about 4 hours under various air pressures before leaving on October 13th, 1884. The results realized were remarkably good. As the engines had not in any way been altered, the valves remaining exactly as on previous voyages and the propeller untouched, an unusually good opportunity was afforded for comparing the performances of the boilers.

The average revolutions of the engines with the natural draught boiler were 56, and the I.H.P. 564.

With the new boiler and forced draught it was found on first trial that 64 revolutions could be easily maintained with the Scotch coal then used.

The indicator on board being out of order no diagrams were then taken, but from diagrams taken on a subsequent voyage, it was found that, with the same load draft, 760 I.H.P. were required to give 64 revolutions.

The steamer put to sea immediately after trial, and made a good run to Trinidad without stoppage. The average revolutions on the voyage were 57, and the consumption 11 tons per 24 hours. The average speed would have been higher but for the formation of clinker and inexperienced firemen. It was attempted on the voyage to run the fires 12 hours without cleaning, but the Scotch coal, which was of inferior quality, contained too large a proportion of clinker to permit this, and eventually it was found to be necessary to clean

fires every 6 hours to keep them in good working order. It may be noticed here that with the Welsh coal of fair quality, which has been used in the steamer for the last 12 months, the fires can be kept in good order, cleaning every 12 hours.

After more than seven months continuous working, in compliance with the request of my firm, Messrs. Scrutton had a report prepared by their superintendent engineer, Mr. Nicolson—who has from the first taken a special interest in this system of combustion—of the results of the working of the new boiler under forced draught up to the date of the report, 11th June, 1885, with a comparison of results from the natural draught boiler under same conditions. In this report the following particulars are given of four voyages made under similar conditions and affording a fair comparison; two being made with the natural draught boiler and two with the forced draught boiler. These are:

With natural draught boiler (1), The homeward run from Barbadoes, arriving in London, January 1st, 1882. (2), The outward run from London to Barbadoes, leaving 20th May, 1884.

With forced draught boiler (1.), The outward run from the Clyde to Trinidad, leaving 13th October, 1884. (2), The homeward run from Barbadoes, arriving in London, 18th May, 1885.

ORIGINAL BOILER WITH NATURAL DRAUGHT.

Voyage.	Draft.		Average Revolutions.	Average I.H.P.	Coal.	Consumption per 24 Hours.	Weather.
	Aft.	Forward.					
	Ft. Ins.	Ft. Ins.				Tons.	
1 Homewards .	20 3	19 3	56	564	Welsh.	13½	Fair.
2 Outwards . .	20 4	18 10	59	Not taken.	Ryhope.	15	,,

NEW BOILER WITH FORCED COMBUSTION.

Voyage.	Draft.		Average Revolutions.	Average I.H.P.	Coal.	Consumption per 24 Hours.	Weather.
	Aft.	Forward.					
	Ft Ins.	Ft. Ins.				Tons.	
1 Outwards . .	20 4	18 10	57	Not taken.	Scotch.	11	Fair
2 Homewards .	20 3	19 6	60	623	Welsh.	9½	Fair, and head wind.

Mr. Nicolson's report in regard to (2) voyage with forced draught boiler and Welsh coal is as follows :—

"Comparing this run with (1) voyage of original boiler, also with Welsh coal, it is "evident that, to have maintained on that voyage 60 revolutions and 623 I.H.P. with that "boiler the consumption could not well have been less than 15 tons per day, so that it "appears to be a fair conclusion that the smaller boiler, with this system of forced combus- "tion, has not only maintained a higher power than the larger natural draught boiler, but has "at same time reduced the consumption of coal with the same engines from 2·24 lbs. to "1·42 lbs. per hour, without taking into account the power for working the fan engine, "which, being supplied from the boiler, should correctly be added to the I.H.P. of main "engines."

On the next voyage out and homewards after the report referred to was made, a still higher result was obtained, as given in the following extract of a letter from Messrs. Scrutton to my firm of 22nd August last :—

"The 'New York City' having completed on 7th August another voyage to the West "India Islands and back to London, we have pleasure in informing you that the results "obtained from your boiler with forced combustion show continued improvement. The "average I.H.P. of the outward and homeward voyages, ascertained by our superin- "tending engineer, is 628, including 6 I.H.P. for the fan engine. The consumption of "Welsh coal has been throughout the voyage 9 tons per 24 hours. The boiler continues in "perfect order. The results altogether are most satisfactory."

This consumption at sea is at the rate of 1·337 lbs. per I.H.P. per hour, with a rate of combustion giving 17·4 I.H.P. per square foot of fire grate. On the last completed voyage a still higher rate of combustion was made by a new engineer, who, without any previous experience in forced draught, ran the engines for some days on the passage to Barbadoes at 62 revolutions, with an indicated H.P. of 706, including fan power, the coal consumption rising to 11 tons per day, this rate of combustion giving 19·6 I.H.P. per square foot of fire grate, with a coal consumption 1·454 lbs. per I.H.P. per hour, the coal being reported as "small and inferior."

With an engineer in charge for the first time, this high evaporative power on the moderate rate of consumption stated is good evidence that this system is perfectly safe at sea in the hands of any intelligent engineer, though without previous experience.

The following statement by the late chief engineer of the "New York City," Mr. John Pettigrew, shews further that a greatly higher rate than 19·6 I.H.P. per square foot of fire grate can be obtained by this system of combustion without difficulty. The letter is dated 15th March, 1886, and is addressed to Messrs. Scrutton, from whom I received it; "As requested, I now beg to describe some tests I made of the power of the boiler of the "'New York City,' with Howden's patent system of forced combustion, while at sea under "my charge. As you are aware, I never had occasion to test the utmost power the boiler "could be worked to, but on one occasion I tried, by order, the power required to give "64 revolutions to the engines with the vessel fully loaded, and found it was 760 I.H.P. "I have, however, used occasionally a much higher rate of combustion than was required "to maintain the revolutions at 64 per minute, having frequently, as a feat, begun to "clean a fire with steam on the gauge at 80 lbs., and have raised the steam to 85 lbs., while "blowing off at the safety valves, before the operation was completed. This was done by "simply charging the other two furnaces, after running the pricker through their bars, "with a good supply of fresh fuel and beginning, as soon as the fresh charge was in flame, "the cleaning of the third fire which had been allowed to burn down. As the fan "continued running at its usual speed, and the air supply was cut off from the fire being "cleaned, the air pressure increased and the two fires consequently received a greater "supply which, with the heavier charge of fuel on the two furnaces, was sufficient to "increase the combustion so as not only to maintain the pressure, but to increase it, as stated, "until steam blew off at safety valves at 85 lbs. pressure. The engines during this time "were left running at their usual speed, no change being made in the admission, and as "the steam increased in pressure the engines would also increase in speed. Though "I never took diagrams when testing the combustion as described, I know they could "not have been working at less than 630 I.H.P."

This increase of pressure, with blowing off at the safety valves and the engines running at full speed, from the combustion of two fires on a total of 24 sq. ft. of grate surface, shews an evaporative power from a square foot of fire grate of probably not less than 30 I.H.P. Those who have experienced the large drop in pressure when cleaning fires in ordinary boilers, will appreciate the value of the facts just stated. These facts also conclusively sustain the very important claim made in my former paper for this system of forced combustion, viz., the absence of any injurious rush of cold air through the furnaces and tubes when a furnace is being operated on, the detrimental cooling effect of which, even in natural draught boilers, is very noticeable.

From such results as those which have been given, and the experience gained from the

continuous working of this steamer for nearly eighteen months, I can now venture to say, with some confidence, that an evaporative power per square foot of grate can be obtained with ease and safety from this system of forced combustion in continuous working at sea, under ordinary conditions, of at least 25 I.H.P. with compound engines at about 80 lbs. pressure, and 30 I.H.P. with triple expansion engines at about 140 lbs. pressure, at rates of consumption not exceeding, whatever less they may be, 1·35 lbs. and 1·1 lbs. respectively per I.H.P. per hour.

I will venture to say further, I have no doubt whatever that higher rates of combustion than those just stated will be attained without difficulty after further experience produces the necessary confidence.

The feature in this system of combustion, which makes such great concentration of power possible in an ordinary mercantile steamer on everyday work, is that of its economy. In taking up this subject of forced combustion a second time, I resolved to stand or fall by the attainment or otherwise of this feature, as it appeared to me the key to the solution of the whole question. That the system should be easily worked on a large scale, and be, as it were, self preservative, were likewise indispensable conditions, as any system of forced draught giving extra trouble in working, and not capable of enduring continuous working over the longest voyages could not be desirable for sea-going steamers.

In my first attempt at forced combustion, in 1862, which was that of forcing air under pressure into a closed ashpit, I was unable to obtain either a workable or an economical system.

To produce complete combustion by this mode of working, at a rate greater than is attainable by chimney draught, the fires must be sufficiently light to allow enough free oxygen to reach the combustible gases above the fuel, but the excess of air necessary to accomplish this is so great as to reduce the evaporative economy much below that obtainable by natural draught.

When, on the other hand, a thick fire was used to prevent the too abundant passage of air through it, the smoke therefrom was excessive, and a large quantity of carbonic oxide escaped unconsumed, while there was much trouble with the fire bars owing to the most active combustion being, not on the top of the fuel, but near to the fire bars. Though the evaporative economy was as a rule higher with the heavier than with the lighter fires, it was still less than that obtainable by ordinary chimney draught working.

Such were the results obtained when working this closed ashpit system as a mode of forced combustion.

There were other difficulties, such as requiring to shut off the air when the furnace door had to be opened. If this system is worked so that the air is prevented from passing through the fire bars and fuel sufficiently to produce any pressure in the furnace by reducing the air spaces between the bars, and the chimney draught therefore capable of drawing air by the furnace door, some of the difficulties mentioned will disappear, but, in such a case, though the air may be supplied under the bars by a fan with pressure, it cannot be termed an example of "forced draught," taking this conventional expression to signify, as it does throughout this Paper, a rate of combustion, unattainable by chimney draught, produced by an artificial supply of air.

This description of my first attempt at forced combustion, on a system which doubtless had been tried by others before me with like results, and which has been since brought out to my knowledge as a new system at least once every decade, will enable my present system, and the manner in which it was worked out, to be better understood.

My first efforts at improvement were directed to obtain a workable and controllable furnace, with air pressure above the furnace bars as well as below, and with this important practical provision, that when a furnace door was opened the air pressure would cease, and a rush of air be prevented from entering into the furnace when it was being operated on. This I accomplished by devising the air pressure casing to cover both the furnace door and ashpit, using an inner furnace door as the proper door of the furnace, and the outer door for retaining the air pressure between the two doors when closed, and for relieving the pressure when opened. This first stage made a workable furnace, but not one properly controllable or highly economical. To effect these further important features, I found it necessary to separate the admission of air above the fuel from that below the fuel, so that the admissions could be made in different quantities and with different pressures at command. This was secured by the division in the air casing separating the ashpit from the air casing over the furnace door, and by the use of separate air-regulating valves for these respective parts of the furnace. These additions made the furnace both workable and controllable in the highest degree, and capable of effecting, if rightly used, the highest possible economy in combustion when, in conjunction therewith, the supply of the air for combustion is heated by the escaping waste gases to the greatest degree attainable therefrom.

This heating of the air for combustion by the waste gases is an element of the first importance in effecting a high economy of fuel in forced combustion, and the one which makes possible or increases the beneficial effects of other combining elements. The limits of this Paper prevent any detailed description here or quantitative illustrations of the various beneficial effects of the heated air.

These are not to be measured only by the direct recovery of so much heat that otherwise would be lost, but also by their effect in increasing the average temperature of the furnace, which again not only adds to the evaporative efficiency of the boiler, but also allows of a more facile and rapid union of the oxygen of the air with the gaseous products and the carbon of the fuel, so that actually *less weight* of hot air than of cold is required to effect the combustion of an equal weight of fuel in a given time. This further raises the degree of evaporative economy and power of the furnace, making an economy and rate of combustion possible which otherwise would be unattainable.

In the "New York City" the recovery of the heat from the escaping gases has not been effected to the extent it could have been, so much having been sacrificed by the adoption of proportions, as already explained, to ensure certain results by chimney draught. That a still higher economy in fuel could be obtained from the boiler of the "New York City" by utilizing a greater proportion of this heat is therefore absolutely certain.

The air of combustion in this steamer is heated directly by the waste gases in the heating chamber from 180° to 200° above temperature of stokehold. This is further increased by its passage through the furnace front plate and interior air boxes, which are preserved by the air carrying off their heat into the furnace, which it enters at a temperature probably averaging about 450°.

I take the opportunity of referring here to the remarks made by Mr. Milton in his Paper, read at last year's meetings, "On the Efficiency of Steam Boilers," on the disadvantage, as he supposes, of my practice of heating the air of combustion by the waste gases instead of the feed water; estimating, as he does, the abstractive powers of water and air to be as the products of their specific gravities and specific heats, thus making water 3,500 times more efficient than air. If Mr. Milton's error had been only 1,000 times less than it is—that is, if water had been even 3·5 times more efficient than air, I would probably not have been now addressing you, as the success of my system is largely due to the fact that air, as I use it, is a greatly better abstractor of heat than water passing through a feed-heater.

I am able to give the relative values in this respect from actual trial sufficiently correct for practical purposes. In the "New York City," the heat abstracted by the air for combustion from the escaping gases in the air-heater, on ordinary sea working, averages 190°. Taking the air used for combustion at 18½ lbs. per lb. of coal we have 190° × 18·5 × ·238 = 836·57 units of heat directly recovered by this air from the waste gases per lb. of coal consumed.

In a boiler which I lately tried, where the whole of the escaping gases passed through a horizontal multitubular feed heater, and where the water evaporated from 42° was 10 lbs. per lb. of coal consumed, the temperature of the feed water, leaving the injector at 114°, was raised, on an average, only 25° in passing through the feed-heater. This gives 10×25°=250 units of heat recovered by the feed water per lb. of coal consumed.

This does not, however, exhaust the case in favour of the air as a better heat abstractor, for the feed-heater referred to had a tube surface of 304 square feet with only 75 lbs. of water passing through it per minute at a velocity of ·025 feet per second, while the "New York City's" air-heater has only 230 square feet of surface with an average of 275 lbs. of air passing through it per minute at the velocity of 25 feet per second. These facts will enable Mr. Milton to readjust his figures as to the comparative efficiency of water and air as heat abstractors.

Before adverting to the important elements of proportioning the quantities of air for combustion and the proper manner of bringing the air in contact with the fuel, permit me to refer to some vague ideas and curious misapprehensions which appear common in regard to such points.

In some recent Papers bearing on the subject, it appears to be taken for granted that all that is required to secure certain temperatures in, and results from a furnace, is the supply of a given weight of air for a given weight of combustible, the resulting temperatures being in an inverse ratio to the weight of air supplied. An example will be found in the same Paper of Mr. Milton to which I have already referred, where, founding his argument on a table in Dr. Rankine's Manual of the Steam Engine, Mr. Milton goes on to say,—"If "the temperature of the atmosphere is 60°, and the fuel is burned by just so much air as "contains the necessary amount of oxygen for combustion, viz., 12 lbs. of air per lb. of "fuel, the resulting temperature of the products of combustion will be 14·640°. If 18 lbs. "of air are used the temperature will be 13·275°, and if 24 lbs. of air the "temperature will be 12·500°." I find elsewhere in a similar Paper, minute calculations worked out from similar hypothetical and theoretical bases, of results expected from boilers with given proportions of grate and heating surface.

Calculations of furnace temperatures and effects on such bases are misleading and delusive.

The temperatures taken from this Manual are merely the theoretical values of the combustion of pure carbon at the instant combustion is complete, on the hypothesis that the proportions of air stated could be supplied without any modifying elements intervening —conditions impossible in furnaces—and were not intended by Dr. Rankine to represent actual temperatures in furnaces. It is quite gratuitous advice for anyone to say, use forced draught with certain proportions of air to fuel; or, use in ordinary combustion certain proportions of air to fuel, and certain results will follow, without such an one showing how the things recommended can be done in practice.

As regards forced draught, I have already mentioned that, my system excepted, I have found its application to boiler furnaces to be more wasteful than chimney draught.

In the furnace of a steam boiler, worked in the ordinary way with the respective air admissions mentioned by Mr. Milton, the relative resulting temperatures would actually be reversed. If 12 lbs. of air per lb. of fuel were introduced into a boiler furnace in operation in the ordinary manner, a much lower temperature than is usual in boiler furnaces, and a more wasteful combustion, would, for obvious reasons, result.

If 18 lbs. of air per lb. of fuel were admitted in an equal time to the same furnace a considerably higher temperature and less wasteful combustion would follow, and with 24 lbs. of air both temperature and economy would be higher still.

I am led to the conclusion that 24 lbs. of air admission would give the best result, from the fact that 24 lbs. of air admission appears to be about the average in well-managed furnaces worked in the ordinary manner.

Now, the reason of this is not because such a large proportion of air is desirable, but because it is necessary in furnaces worked in the ordinary manner, for if the same active combustion could be effected in the same time with less air, a proportional increase of temperature and economy in fuel would inevitably follow. Universal experience, however, shows that to obtain a certain evaporative power from a boiler worked in the ordinary way about 24 lbs. of air must be used. Had it been possible by merely shutting off a certain quantity of air from a furnace to have obtained a higher temperature and better results, it would have been discovered long ago. The apparent anomaly, of the worse practice theoretically, giving the better result, arises from practical conditions due to the manner in which the air

is admitted to the furnaces and the behaviour of the gases in combustion, which, however, cannot be gone into in detail in this Paper.

Temperature in furnaces does not depend primarily on the weight of air used per lb. of fuel consumed, even when that combustion is judiciously effected, but on the quantity of fuel brought under combustion in a given time and space, the greater the quantity consumed the higher the temperature.

Returning to the consideration of the effect of the manner in which fuel is burned, I would point out that though it is impossible in the large scale of a boiler furnace to reduce the proportion of air very closely to the theoretical quantity sufficient for the complete combustion of the fuel, yet a very large reduction can be made on the proportion now used by a different mode of making admission. Except for the hydro-carbon gases, which apparently cannot be consumed without a considerable excess of oxygen, very little excess would be required for the complete combustion of the solid carbon by the adoption of effective means for combination. A large dilution of the carbonic acid in the furnace by admission of excess of air is only necessary when furnaces are worked in the ordinary manner. What is wanted is to bring the air for combustion *simultaneously* in contact with the gaseous and solid fuel *over the whole surface of the furnace* at a velocity that will insure its intimate mixture directly with the fuel. In such circumstances a greatly reduced proportion of air will suffice for combustion, more especially if the mode of effecting combustion is at same time so arranged as to gasify the carbon to a large extent.

I have endeavoured in carrying out my furnace operations to approximate as nearly as possible to these conditions, and to work the furnace as, what may be termed, a combined quick-gasifying and complete-combustion furnace, by the following means:—The air in the ashpit, with a given area of air-space through the fire bars, and a given average depth of fuel, is maintained at a pressure designed to pass a quantity of air through the fuel sufficient to gasify it and bring it to the surface largely in the form of carbonic oxide. The air in the casing between the two furnace doors is maintained at a considerably higher pressure than in the ashpit, and is thus received by the distributing boxes inside the furnace plate and inner furnace door. The air then, at a considerable temperature, and at a high velocity, issues in minute streams from small holes in the interior side of the air boxes, their aggregate area being proportioned to the normal work of the furnace, and their position arranged to cause the air to strike the fuel with force equally over the surface within the limits of the fire bars, as represented in Fig. 3.

By means of these differential pressures used as described, the weight of air required for the complete combustion of a given weight of fuel can be made much less than is necessary in an ordinary furnace, while with the complete stage of combustion being chiefly on, or above, the surface of the fuel, a clear white flame and intense heat is generated where most effective for radiation, and most innocuous in its effect on the furnace bars.

This account of the manner in which I have attempted to carry my ideas of combustion in furnaces by forced draught will shew that it has been by no hap-hazard plan or fortuitous circumstances that the unique economy, high evaporative power, reduced wear and tear of boilers and fittings, and other important advantages as exemplified in the "New York City," have been secured, but by a careful working out of what are evidently correct principles in a practical manner.

The practical difficulties, which have arisen from working this system at sea over eighteen months by inexperienced and sometimes careless firemen, have been greatly less than I anticipated. They have in no case been peculiar to the system of working, and are of so little consequence that it is unnecessary to occupy time in mentioning them.

I may, however, appropriately refer here to a circumstance which occurred in May last in the Highland paddle steamer "Grenadier," built by Messrs. Jas. and Geo. Thomson, Glasgow, which I find has been widely reported as a "failure" of my system. The two boilers, which are of the "Navy" type, were made for natural draught working, but were fitted at the owner's request with my system of forced draught to be used when required.

The furnace fronts and doors, with air valves, were fitted by my firm, while the air reservoirs, heating chambers, air pipes, fans and engines—one for each boiler—were fitted by Messrs. Thomson to dimensions and sketches which I supplied. As the total I.H.P. required from each boiler was only 550 at the outside, and the size of each fan and engine was the same as used in the "New York City," and giving 760 I.H.P. there without trouble, I had every reason to look for a sufficient air supply.

On the trial, however, there was some difficulty with the frictional driving gear of the fans, used on account of limited space, so that, with the insufficient driving and probably some obstruction in the air-pipes, at no time during the three hours of the trial was there more than $\frac{1}{8}$ of an inch water pressure registered in the ash pits. As full power could not be produced from shortened grates, with restrictions in tubes and air heaters, with this inadequate pressure,—though it sufficed to give 38 revolutions to the engines, 41, I believe,

being the highest attained with full power—Messrs. Thomson, in order to give a full-power official trial two days after, removed the air heating tubes and lengthened the grates. After this the steamer was put on her station, and no further opportunity remained to rectify the forced draught arrangement.

I need scarcely say that my system was no more at fault in this case than an engine would be, if it failed to run at a velocity which required 60 lbs. steam pressure, if only 10 lbs. were supplied.

In my Paper of 1884, in referring to other systems of forced combustion within my knowledge, I pointed out the grave disadvantages which must attend the use of the closed, or air pressure stokehold system, used so extensively in our war-ships. Since then I have had occasion elsewhere to refer still more strongly to this system as one essentially injurious to boilers. It therefore devolves upon me to show sufficient cause for holding such opinions. The importance of the subject will excuse my doing so at some length.

It is a singular circumstance that though this system has been fitted in a number of vessels for several years, I believe I am strictly correct in saying that for forced draught rates of combustion it has never yet been proved by 24 hours' continuous working at sea.

The case of those steamers belonging to Mr. Alfred Holt, which are fitted with closed stokeholds and which have been worked more or less at sea, is not a case in point. These steamers are not worked at a forced draught rate of combustion.

Mr. Holt has favoured me with particulars of two of these steamers, the "Hector" and the "Anchises." The "Hector" developes 780 I.H.P. from 90 square feet of grate in a boiler having six furnaces and 2,504 square feet of heating surface, and the "Anchises" 900 I.H.P. from 90 square feet of grate in a boiler with six furnaces and 3,101 square feet of heating surface, being respectively 8·66 and 10 I.H.P. per square foot of grate per hour. With this rate of combustion the injurious effects arising from this system of forced combustion are not developed. The boilers used by Mr. Holt are also, from their design, peculiarly fitted to withstand the injurious effects of the cold air admission.

The Holyhead mail steamers are also fitted with this system. The boilers are, like those of Mr. Holt's steamers, quite capable of supplying full steam by natural draught, and the supply of air by the fans is only used when the weather or other circumstances are unfavourable for a good natural draught. The run each way occupies about $3\frac{1}{2}$ hours, and

the steamers are laid up one week out of every three. The rate of combustion gives about 6·75 I.H.P. per square foot of grate, but this is with low pressure simple engines.

These examples and similar ones in America, where working a fan in an open stokehold has been long in use, do not therefore fall within the cases of forced draught, so that the question, whether boilers can be worked with this system, when the steamers are put to the use for which they were built, remains yet to be proved. Meantime, in the absence of direct proof, the effects of this system can only be judged of from experience obtained from analogous cases, and my experience of such cases, which has been on a somewhat extensive scale, leads me decidedly to the conclusion that this system is unfitted for the ordinary working of boilers at sea.

The cause of injury to the boilers, common to both the closed stokehold system and to the cases within my experience, is that of the sudden cooling of the interior of the boiler by a rush of cold air immediately after being under a high furnace temperature, but, in those boilers in which I have seen the ruinous effects of this sudden change of temperature, being worked by natural draught, the rush of cold air through the boiler was much less in velocity and volume than obtains in boilers with closed stokeholds under air pressure.

The boilers to which I refer were fitted with their engines by my firm a few years ago, in steamers of over 4,000 tons, whose names it is not necessary to give here. The boilers in each steamer were ten in number made to the owning company's specification on a certain patent design somewhat of a locomotive type, but without sides or front to the fire-box part, which was formed of brick-work. The maximum working pressure was 125 lbs. and the boilers were designed to supply steam to work the engines up to 3,300 I.H.P.

On trial, with the boilers quite clean and containing fresh water, they supplied steam steadily and well, and worked continuously over the whole day without the slightest leakage at any part or sign of injury. Starting thus on a voyage to Calcutta, the power being reduced to about 2,100 I.H.P., the boilers ran for a number of days without trouble, but as soon as the formation of a very slight scale on the tube plates began and the water increased in density by replenishing the waste from the sea, the increase of temperature on the tube plates exposed to the flame rendered these so much more sensitive, that on the opening of a furnace door the sudden rush of air across the furnace, a distance of 6 ft. 6 ins., struck the tube plate, with a difference in temperature sufficiently great to cause a sudden contraction of the tube plate and unequal contractions of this thicker tube plate and the thinner tube ends, so that a general leakage at the tubes took place. This leakage

once begun could not by any means be stopped, so that the dust and soot from the fire adhered to the damp plate, and in a short time accumulated to such an extent as almost to close up the tubes and reduce the power of the boilers to a small fraction. The only course then left was to draw the fires, clear and tighten the tubes with an expander, and then go on again to repeat the above process, sooner or later, even with the most careful management. To work these boilers, without continual trouble and great expense, even at a further reduced power, it was found necessary to use fresh water only and to protect the tube plates as far as possible by loopholed brickwork. A large multitubular boiler, having two furnaces 3 feet in diameter, was kept employed in supplying steam from sea water for condensation in order to make up the entire waste from fresh water. Notwithstanding these precautions and after many expensive repairs, the boilers of the first of these steamers were, after 18 months' working, so much injured that new boilers of ordinary form were ordered to replace them. Before their completion, this steamer was lost in a voyage across the Atlantic, and the new boilers were fitted into the second steamer a few months thereafter, the boilers of this latter vessel having suffered in a like manner to that described.

These boilers, though of a type more liable to injury than the usual marine type, show what would undoubtedly take place in any boiler in which a large volume of comparatively cold air suddenly impinged on the interior plates, especially the tube plates, which, until the moment of impact, had been under a high furnace temperature. That the vast volume of air rushing through the furnace of a boiler worked on the closed stokehold system, with forced combustion, would strike the tube plates cold enough to cause serious damage, may be inferred from the area of the volume and its velocity, the former that of the furnace door, the latter, 60 to 70 feet per second.

This conclusion is indeed not left to mere inference—it is a fact within the experience of most sea-going engineers working with chimney draught, that when considerable scale accumulates on a back tube plate of an ordinary marine boiler, if a furnace door is suddenly opened under certain conditions of draught, the rush of cold air causes the tubes to leak. I called attention two years ago to the facts recorded by Mr. R. J. Butler in his Paper of 1883, "On the Steam Trials of the 'Satellite' and 'Conqueror' under Forced Draught," when even after trials not exceeding three hours' duration, with boilers perfectly clean and containing fresh water,—and producing less effect in I.H.P. per square foot of grate than the boiler of the "New York City" has been working at continuously at sea for 18 months with salt water,—the boilers in both steamers were leaking at the tubes, a result seriously significant. It will be remembered that the boilers just described, which

suffered so greatly from the effect of the impact of cold air, did not show the slightest injury on a prolonged trial nor for some days after they had been at sea, while in the "Satellite" and "Conqueror" a few hours' trial, under the most favourable conditions, produced the injurious effects described.

I am aware it is stated that it is not intended to work the steamers with this forced draught at sea unless emergencies arise to require it, and the boilers are made large enough to supply steam for full power by natural draught. If so, then the proper advantages of forced combustion, reduction in weight of machinery and space occupied, are given up, and the vessel cumbered with an expensive and troublesome arrangement, well known to be at least wasteful in fuel, for the prospective advantage of it being used on emergencies. But I must ask, will even this prospective advantage be realized? I am convinced it will not. I believe that after a steamer fitted on this system has been at sea for some time with natural draught, and the boilers have accumulated a slight scale, if it were attempted to use forced draught on an emergency, the boilers would soon be rendered useless, and the vessel left helpless.

Another difficulty I cannot avoid reference to, which is never experienced on trials of a few hours or on short sea passages, but which must be faced at sea with continuous working, is that of cleaning fires. With the large consumption required for this system of forced draught, fires could probably not run for more than six hours without cleaning. Would it be possible to clean a fire with a volume of cold air of the area of the furnace door rushing through the furnace and tubes at the velocity due to $1\frac{1}{2}$ ins. or 1 in. water pressure? Supposing the boiler was not injured by the terrible scour of cold air, it would certainly be cooled down to a serious extent by the furnace and tubes, for the time being, acting as a condenser. The only alternative would be to reduce the air pressure, for the time being, to that of the atmosphere, if this could be done with safety in a boiler room in connection with others through a common funnel. Take a steamer having thirty-six furnaces divided into four separate air pressure compartments, cleaning fires as mentioned, six furnaces would require to be cleaned every hour. This would place at least half of the boilers continuously under very low combustion; so that whatever alternative, whether this or the worse, was adopted, the cleaning of the furnaces on this system under continuous working, could not be other than a serious practical difficulty.

I have thus given, in somewhat full detail, the reasons which have led to the strong convictions I have expressed in regard to the dangerous unfitness of this closed stokehold system of forced draught for sea-going steamers.

Putting aside altogether the question of injury to the boilers, and recurring to the fact that the steamers of the navy which are fitted with this forced draught system have boilers of the usual size, so as to steam full power by chimney draught, let us assume that with forced draught the boilers of a steamer of the larger class could supply steam to drive the engines continuously at sea at a power 20 per cent. above that of full chimney draught power, without trouble in cleaning fires or the slightest injury to the boilers. I believe I make no over-statement when I say that, even in such a case as here assumed, the same steamer fitted on my system of forced draught, and with the boilers required for equal power, would steam at equal speed three times the distance it could do with the closed stokehold system of forced draught. This is owing, first, to the greatly reduced size and weight of boilers, water and fittings required with my system, leaving room for larger carrying space; and, secondly, to the much smaller consumption of coal required to produce the same power.

The vital importance of such an effect in the reduction of weight and space and coal consumption in war-ships does not require illustration here. The difficulty in building ironclads on favourable forms for economical propulsion, and securing at the same time sufficient protection in thickness of armour plate, would be greatly relieved by the reduction of 1,000 tons in the weight of the necessary machinery and coals in one of the larger ironclads. Still keeping out of view the vital difference between using a safe system adapted for everyday work, and an unsafe system unsuited for everyday work, the advantages of possessing high-speed cruisers, able to run continuously with ease at the highest speed, and to keep the seas, as I have said, for thrice the time they could with the present system, I leave to be estimated by naval officers and others experienced in handling such ships.

For steam yachts, the use of my system of combustion would make a speed possible, with a reduced weight of machinery, quite unattainable with present arrangements, while the absence of smoke, the perfect control of the steam—so that blowing off at the safety valves under any circumstances can be prevented—also, the power to reduce the evaporation at will to the lowest point sufficient to turn the engines, and to lie at rest for many hours with the combustion practically stopped, and thereafter to go on at full speed at one minute's notice, are all advantages of a very important character not only for yachts, but for any class of steamship and of special importance in war-ships.

In my Paper of 1884, I gave an example of the reduction in size and number of boilers which, on my system, could safely be made on such a steamer as the "Oregon," then the most powerful passenger steamship afloat. The boilers in that ship were nine in number,

16 ft. 6 ins. in diameter, and 18 ft. in length, each having eight furnaces, 3 ft. 6 ins. in diameter, or 72 in all, with an aggregate fire grate surface of 1,512 square feet. The boilers with which I proposed, on my system, to maintain an equal power, viz., 12,000 I.H.P., were six in number, 15 ft. in diameter × 18 ft. in length, with six furnaces in each, 3 ft. 9 ins. diameter, or 36 furnaces in all with an aggregate fire grate surface of 641 square feet.

It will probably be remembered that, when I proposed to give an equal power with such a reduction in the boilers of this ship, it was considered to be quite impracticable. I need only point to what has been accomplished continuously for a period of 18 months in the cargo steamer "New York City," on much longer runs, and where a high power and speed are not sought for or attempted. 12,000 I.H.P. from 641 square feet of fire grate is only 18·75 I.H.P. per square foot, a rate which has been surpassed in the "New York City."

It is therefore quite evident that a considerably greater reduction in boilers than I then proposed could be made in such a steamer as the "Oregon" with an organised staff, and arrangements for the attainment of a high speed. In concluding with an illustration of the commercial advantage of such an arrangement as I proposed in such a vessel as the "Oregon," I will, to keep well within very safe limits, assume the boilers to be in size and number what I formerly proposed, and instead of the lower rate of the "New York City" I will take the consumption at 1·5 lbs. per I.H.P. per hour, and also instead of assuming the rate of the "Oregon," with her natural draught boilers when developing at sea 12,000 I.H.P. at 2·6 lbs., will assume a rate of only 2·28 lbs. at that power.

These rates give a reduction in coal consumption of exactly 100 tons per day in favour of the forced draught boilers. Taking these rates of consumption and numbers and sizes of boilers as a basis of comparison, I find from a calculation of the weight of the reduced number and size of boilers, including water, mountings, uptakes, piping, flooring and other necessary fittings, that they could not be less than 450 tons under that of the original boilers. The reduced space occupied by the fewer boilers under main deck gives room for 1250 tons cargo of 40 cubic feet, while the 'tween decks and deck house, which would be set free for passengers, would accommodate at least 80 first and second class passengers.

The coals carried per voyage each way being 800 tons less, would leave additional room for 800 tons of cargo of 40 cubic feet per ton.

The more convenient coaling arrangement secured—not only by the reduced number, but by the smaller size of boilers—and the consumption, less by 100 tons per day, should reduce the number of firemen and trimmers by not less than 60, and thus set free berth room for at least 40 steerage passengers.

Taking ten round voyages per annum between Liverpool and New York, with seven days in each port and eight days each way at sea, or 30 days for each round voyage, there would be per annum 160 days steaming, 140 days in port for loading and discharging, and 65 days laying up for yearly overhaul and repairs.

Supposing the extra passengers and cargo are obtained, the savings and increased earnings at the following rates, which provide for a further decrease on the present rates already abnormally low, would amount to—

On Round Voyage.

Saving in fuel @ 10s. per ton, put in bunkers Liverpool and New York—	1,600 tons @ 10s.	£800	0	0
Cargo, weight and measurement	4,100 tons @ 20s.	4,100	0	0
160 1st and 2nd class passengers, less cost of victual, £10 each		1,600	0	0
80 3rd class passengers, less cost of victual, £3 each		240	0	0
Pay and victualling of 60 firemen @ £5 per month		300	0	0
	Total for round voyage	£7,040	0	0
	Total for ten round voyages	£70,400	0	0

DISCUSSION.

Mr. J. Wright: Sir Nathaniel and Gentlemen, as the two Papers which we have just heard read are on one of the most burning subjects of the day, connected with Marine Engineering, no doubt many gentlemen here wish to take part in the discussion, and so I shall make my remarks as brief as possible. Mr. Sennett has fairly stated the case on behalf of the closed stokehold; his statements are all based on very carefully taken facts, and the few suggestions that he has made beyond the facts are put forward modestly as suggestions, and not as he says with any attempt to dogmatize. Mr. Howden, on the other hand, is not content with having put his case very favourably for his system, but he has entered the enemy's camp and demolished the rival system most completely according to his own

notion. If Mr. Howden had been content to give us a description of his system as applied to the "City of New York," I should have been disposed to confine my remarks to congratulating him on the very considerable success he has attained in that ship. Her old boiler may have been bad and badly worked, but there is no doubt the new boiler he has supplied is a very great improvement on the old one. He has been, however, so very bellicose, that I am obliged to lay before you some statements and calculations on the other system with which I am more particularly connected. I had an opportunity of getting a copy of the Paper early this morning, and I have made a few calculations. First of all I should like to draw your attention to the very small size of fire grate in the boiler, and I find on going through Mr. Howden's Paper that he has almost entirely based his calculations upon the area of fire grate, ignoring entirely, or nearly so, the question of cubic capacity of boiler, and weight of boiler. I want to give you a few particulars, comparing them on the same basis. In the first place I should like to say that I think his system can scarcely be called forced combustion at all, because it is really a very moderate rate of steaming. I will take the power which he has given at the bottom of page 186, 623 horses and 9½ tons consumption per day, and taking that in a fire grate of 36 square feet, it is only a rate of consumption of 24½ lbs. of coal per square foot of grate per hour. I will take the highest rate given, namely, 11 tons per day, that, on the same basis, is only 28½ lbs. of coal per square foot of grate per hour. With ordinary draught it is a very common thing to burn from 21 to 22 lbs. of coal per square foot of grate per hour, on a very much larger proportion of grate than there is in that boiler; with the steam jet applied to boilers we used to burn from 28 to 30 pounds without any difficulty. In the "Howe" which was mentioned in Mr. White's Paper the other day, under forced combustion, she burnt 33½ lbs. of coal per square foot of grate per hour on a very large fire grate. I think I have shown you the rate of combustion is a very small rate, and it ought, therefore, to be a very economical rate. I should like now to call your attention to the proportion between the fire grate and the heating surface. The ordinary proportion of fire grate to heating surface in naval boilers, and I believe also in the ordinary boilers of Merchant Ships, is from 27 to 30 square feet per square foot of fire grate. In the boiler of the "City of New York" the proportion is between 42 and 43 square feet, I could not calculate exactly, because the boiler drawing does not show it sufficiently; that is to say, the proportion of heating surface to fire grate in the "City of New York" is 45 feet more than the ordinary proportion, and it is bound, therefore, to be a very economical boiler. I should like now to refer to the question of the cubical capacity of the boiler in comparison to the amount of power given out. The cubical capacity of the boiler of the "City of New York" is 1,693 feet. I will just take two cases, to save time, of Naval boilers, one the "Amphion" and the other the "Howe." The cubic capacity of the "Amphion's" boiler is 1,331 feet; one boiler of the "Howe" 1,344 feet, that is to say, the "City of New York" boiler is 27 per cent. greater than the boiler of the "Amphion," and 26 per cent. greater than the boiler of the "Howe." I wish you would keep these percentages, gentlemen, in your mind. The next thing is the horse power which has been developed. There seems to be a most unfortunate reluctance on board the "City of New York" to take diagrams, but we have got a few statements. I will take the power given at the bottom of page 186 in the first instance, 623. That, of course, Mr. Howden will say is a very low power; I will take it for granted it is, and go to a higher power by and bye. Taking the 623 as the power given out by the "City of New York," one boiler of the "Amphion," 27 per cent. smaller, with natural draught, gave 706 horse

power; one boiler of the "Howe," with natural draught, gave 644 horse power, and one boiler of the "Howe," with forced draught, gave 977 horse power. Taking the horse power per cubic foot of boiler it comes to this, per cubic foot of boiler in the "City of New York" it was 368, in the "Amphion" with natural draught 53 and in the "Howe" with natural draught it was 479, in the "Howe" with forced draught the horse power per cubic foot of boiler was 727. That is to say, the horse power given out in the "Amphion" with natural draught per cubic foot of boiler was 44 per cent. more than in the "City of New York" with Mr. Howden's forced combustion. In the "Howe" with natural draught it was 30 per cent. more than the "City of New York" and in the "Howe" with forced combustion it was 97½, in round numbers twice as much. The highest power given for the "City of New York" is 760. I do not see that there is very exact evidence given as to the 760, but we will take it as being the most power the boiler can give. Taking it on the same basis of cubic capacity of boiler, I find that that gives 449 horse power per cubic foot of boiler, that is to say the "Amphion's" boiler, with natural draught, gave 18 per cent. more than the maximum power of the "City of New York" with forced combustion; the "Howe's" boiler with natural draught gave 6⅔rds per cent. more than the "City of New York" maximum, and the "Howe" with forced combustion gave 62 per cent. more than the "City of New York" with forced combustion. I should like just to read to you a few words at the top of page 200 of the Paper: "I believe I make no over-statement when I say, that, even in such cases as here assumed, the same steamer fitted on my system of forced draught, and with the boiler required for equal power, would steam at equal speed three times the distance it could do with the closed stokehold system of forced draught. This is owing first to the greatly reduced size and weight of boilers, water and fittings required with my system, leaving room for larger carrying space, and secondly, to the much smaller consumption of coal required to produce the same power." The figures I have given you, Gentlemen, are, I believe, practically correct, and I will leave you to draw your own conclusions whether or not Mr. Howden's figures and Mr. Howden's statements do not require some correction.

Mr. J. I. Thornycroft: Sir Nathaniel Barnaby and Gentlemen, I was much struck with Mr. Howden's Paper, but I am afraid the claims he makes are too extensive. I agree with Mr. Wright that the figures may require a little correction. The first thing that struck me on looking at the section of the boiler was, as Mr. Wright has told us, that the furnace is very short. I think in that Mr. Howden gets an advantage, because the fire can be better attended to, and the boiler is evidently large in proportion to the coal burnt. Diagram No. 3 I would like to call attention to, because if, as is usually the case, a very considerable quantity of air goes through the fire, it is quite impossible that those rays of air going so kindly like rays of light straight to the fire could go in that way without being deflected, therefore, I think Mr. Howden must be a little mistaken in what takes place. Mr. Howden seems to be of opinion that that is the only way of burning coal well. Now, I think, the locomotive Engineers have had some experience of burning coal well, and they do get a very good duty out of the boiler, but in this case they keep the fire door open to some extent and they let in the cold air, but they deflect it down where they have plenty of depth and a high bridge, which causes the gases from the fire to meet the cold air, but unfortunately I am sorry to say in the locomotive boilers I have had the designing of, we have not been at liberty to put the locomotive firebox down

low enough, because we should have got down out of the bottom of the boat, but in the locomotive firebox, where the firebox is short and deep, the coal is tumbled in forming a thick fire and the air may be sent in at a very high velocity, there, I think, the combustion is very perfect, and I am not sure there is much to be desired. If the air can be heated as Mr. Howden proposes, there is no doubt heating the air is an advantage, but as to the fact that combustion would be so much more perfect, that I am not so sure about, because it is said, and I believe truly, that at very high temperatures the gases do not combine better, and you do arrive at a temperature when they no longer combine. In the boiler before us one thing that struck me was the very small opening that the air comes in at, but I do not know whether it is to scale. One thing I found necessary where really much coal has been burnt in the fire-grate was, to have the passages for the air so large as to use a moderate velocity and not to cut the ashes about into waves in the ashpits, which are liable to pile up and take fire; and if, in a rapid stream of air, you have the ashes under the bars take fire, I need not say what the result is. I think what Mr. Wright said as to the boiler only doing a low duty in this case, applies to the bulk of the boiler which, of course, means per weight of boiler, for everybody's boiler would weigh about the same weight for the same pressure. Mr. Howden proposes reducing the boilers very greatly. There he will damage the result obtained. Now he has a large boiler to do the work; then he will have a small boiler to do the work, and then he must lessen the economy. The economy stated in the Paper is very good, and I only hope, that when more diagrams are taken, it will be found that really so high a duty has been attained, because I take it to be a very great engineering success, if we can get engines to do the work with a little over a pound of coal. I do not know that I need detain you any longer, but I must say something in favour of the closed stokehold, because it does really seem to be the simplest way of forcing the fires. It does not heat the air in the way Mr. Howden has stated, where he uses a special apparatus for heating it, but I am not sure whether there is not something to be discounted off that, because any elaborate heating apparatus you put in the chimney to heat the air will be subject to considerable oxydation, and you will have to keep that in repair: then again, if you close your stokehold, all the air that comes in takes up the heat from the boilers round about and the temperature is more or less raised. Any heat that escapes from the boiler must be taken up by the air and it all goes into the furnace more or less, so I do not think the heating apparatus can be credited with so much advantage as he has given to it. I do not think the air is nearly so much heated in Mr. Howden's apparatus as he imagines. Mr. Howden describes the fire as a sort of Siemens' Gas Generator, as a large quantity of air is blown on the top, and he describes the combustion as being at the top of the fire and not underneath in the ordinary way. I am not sure whether that takes place or not. I am a little doubtful whether the combustion in these cases is very different to what it is in an ordinary fire.

Mr. A. F. Yarrow: Mr. Sennett states that on the trial of the "Mersey," 16·61 indicated horse power was obtained per square foot of fire grate, and 21·7 indicated horse power per ton of boiler. He supplemented that statement by remarking that this was the best result that had come under his notice. I presume he refers to the boilers of large vessels.

Mr. Sennett: Yes, ordinary marine boilers.

Mr. Yarrow: For the sake of comparison, it may be interesting for me to state that a short time since we designed and successfully worked some boilers of very nearly the same power and heating surface as the "Mersey" boilers; if you refer to page 180, you will see each of the "Mersey" boilers had a heating surface of 1,950 square feet, and our boilers had each 2,000 feet of surface; it will be seen, therefore, that the heating surface of the two types of boilers were practically the same. Now, from our boilers we obtained 1,250 horse power, with a weight of twenty tons, including water; giving 62·5 horse-power per ton of boiler. I mention these facts simply for the sake of comparison, from which it will be seen we obtained three times the power for the same weight. Now, referring to the grate area in the boilers of the torpedo boats which we are building for the British Government, we generally adopt 28 feet of grate surface; but in one, for the sake of experiment, we reduced it to 20, and, as a matter of fact, we did not find this reduction in area practically augmented the air pressure in the stokehold, nor, in fact, the steaming power of the boiler, but it gave greater ease to the stokers in managing the fires, on account of their reduced size. With the 20 feet of grate we got 650 horse power—*i.e.*, we obtained 32½ horse power per foot of grate, which also compares very favourably with what has been done in the "Mersey." I simply mention these points for the sake of comparison, and do not for a moment wish you to suppose that I consider boilers such as I refer to would be equally applicable to those adopted in the "Mersey," but it does show the great gap there exists between the most advanced practice in large vessels and that found in torpedo boat practice. I do submit that this gap could be reduced without practical objection. Now, as regards Mr. Howden's Paper, on page 184 he gives us the comparison between two boilers, the new and the old, and he tells us of the gain he has obtained. It would have been of very great advantage to the Institution if he could have indicated—as there have been so many changes—how much of the gain was attributable to each change; perhaps the most interesting alteration, and the one to which he directs most attention, is the heating of the air; it would be most instructive if he could tell us what proportion of the gain is due to this cause alone. A very simple experiment could be tried to find this out, by letting the air, instead of passing through the heating apparatus, simply be drawn from the atmosphere direct, and brought into contact with the fuel in its cold state, comparing the result with that obtained if the air is first heated. I do not think that experiment would be an expensive or difficult one to carry out, and certainly it would be a very useful one. I certainly think the Institution would be further indebted to Mr. Howden, if he would supplement his Paper by giving us the result of such a trial; all the conditions in the two cases being precisely the same, the temperature of the air only excepted.

Mr. J. T. Milton: I would like to make a few remarks, because my name has been mentioned in Mr. Howden's Paper. In the first place, with regard to Mr. Wright's remarks, the question of forced draught as applied in the navy, and as it has been applied by Mr. Howden, are entirely different things. In the navy the attempt has been made to obtain a very high evaporative power from the boilers irrespective, or almost irrespective of economy of fuel. In this case of the "City of New York" it has only been applied in order to obtain economy of fuel. The conditions are entirely different, and naturally, if you aim at different conditions it appears to me that different means should be adopted for fulfilling those conditions. Now, in Mr. Howden's boiler, as Mr. Wright

has pointed out, he has used a very large proportion of heating surface to grate-surface. That, I believe, is the right direction in which to proceed if you want to obtain economy. Now, Mr. Wright has made some remarks comparing the boiler of the "New York City" with some of the navy boilers as to the amount of power got out of them in proportion to their size. Now, there again the navy boilers and the merchant service boilers have different conditions to fulfil. In the navy, to obtain the power, they cannot afford to occupy the same space that can be given in the merchant service. So that in the merchant service they have very much larger boilers for the same power, worked with natural draught, the extra size being given for the purpose of facilitating cleaning and keeping them in repair. If you look at this boiler it would strike any one who had been used to navy boilers, that the spaces round here between the combustion chambers, and in fact, the whole of the water spaces, were much larger than would be adopted if you were trying to get the same effective power in a smaller shell; in other words, the same effective power could be got in a smaller boiler than this if you chose to surrender accessibility for cleaning. Now, Mr. Howden possibly, rather generously, has left out reading what he remarks about my Paper read at the last session. I stated then, and I believe now, that if you have to employ any heating surface in the funnel to abstract the heat from the waste gases, it is far better to abstract the heat by means of water than by means of air. If we want to cool an iron plate when it is made hot, we never think of putting a blast of air over it, we put it in water or sprinkle water upon it, because water abstracts heat very much more rapidly than air. With regard to the actual effect that Mr. Howden claims to have realized, I think he is mistaken in his estimate of the temperature of the air. I know there is nothing more difficult than to obtain the correct measure of the temperature of air. An ordinary thermometer does not show the temperature of the air in which it is placed, unless under very exceptional circumstances. It is affected by a radiation from all surrounding objects, and it shows practically the temperature of itself and nothing else. Now, in this case, I believe I am correct in saying, I was told so on board the ship where I have seen the actual boiler, the thermometer for measuring the air temperature was placed between the outer and inner casings of the furnace door, and at the front end of the ash-pit. Is that so, Mr. Howden, in that space?

Mr. Howden: The air temperature from the air-heater is not taken at the furnace doors or casings, as Mr. Milton supposes, but in reservoir at place marked A on Fig. 1, Plate XXII., quite away from any influence of radiation. There are other holes for taking temperatures at the furnaces.

Mr. Milton: The chief engineer pointed out the hole in which they put the thermometer, and another hole in which they put another thermometer. In both those positions the thermometer would be exposed to the radiation of some very hot plates; these hot plates, in the first place, and all the underneath surface of the bars and ashes in the second place, and I think, therefore, that they can hardly be taken as showing the temperature of the air. As a matter of fact, I asked one of the men on board the question out of curiosity, whether this plate got hot, as it would do if the temperature of the air got to 300 or 400 degrees, and he said, No, you can always bear your hand on it. I do not know whether it is so. I think that Mr. Howden has been possibly led into error through the difficulty of measuring the air temperature. One point that has been raised

prominently is, whether the system of a closed stokehold or closed ashpit is the preferable system for use. I think that this matter largely depends upon the number of boilers. If you have only one boiler, the closed ashpit is, in my opinion, far simpler than the closed stokehold. With a closed stokehold you have a lot of bother with the doors and air locks, and so on, and it is a very simple matter with the closed ashpit. With more boilers I do not like to say. When you have a great number of boilers in one stokehold, probably the other would be simpler, but when the air once gets in under the furnace bars, I think the effect is the same whether it comes in from the closed stokehold or from a closed ashpit; the chemical effect of the air on the fire cannot be very different. I would like to hear Mr. Howden's testimony on one matter he has mentioned here, as to the probable continued efficiency of boilers worked by forced draught. I am acquainted with the results obtained by the boilers he was speaking of, and I think that, if anything, the description he has given of the trouble they gave is understated rather than overstated. Those boilers, as he said, were boilers fitted on something like a locomotive plan, and in them, because the fires had brick sides, and there was nothing to take away the heat of the products of combustion, the tube plates were subjected to a much greater temperature than they would have been in ordinary boilers, and therefore, the effect on the tube plates is fairly comparable to that likely to be obtained with the higher temperatures occurring in furnaces with forced draught. The tubes could not possibly be kept tight when there was any sea-water used in the boilers at all, and they had to be supplied with absolutely fresh water the whole of their lifetime. It meant a very serious trouble. In this boiler, the "City of New York's," having a small grate, and we will say a double proportion of furnace crown to extract the heat from the products of combustion before they reached the tube plates, that difficulty is met, and I have no doubt it is to a large extent to this abnormal proportion of furnace crown that the continued efficiency of the "City of New York's" boiler is now owing.

Mr. W. Boyd: I think, Mr. Chairman, that like some or many enthusiasts, Mr. Howden has, perhaps, somewhat prejudiced his case by taking a rather too sanguine view of the results that are obtainable from it; but we ought not to forget, that Mr. Howden has shown, that at any rate in this vessel to which allusion has been made, he has really done good and substantial work, and it so happens that like many other occasions in the history of engineering, and in the world's history, the old proverb becomes true that "Necessity is the mother of invention," and the time seems to have come when men's minds are driven in the direction of some further economy being absolutely necessary to make our mercantile steamships even moderately remunerative, and that engineers have come to the conclusion that attention ought now to be devoted to the boiler and the "production" of steam, in contradistinction to the "using" of it. I think it will be within the knowledge of most present, that the time is singularly opportune for the consideration of this question. I find that within my circle of acquaintance, men are prepared to deal with this question of forced draught in a way that they would not have been prepared for a few years ago, and so far as it goes I think we are indebted to Mr. Howden for his perseverance in his efforts to produce this economy, and for the very valuable Paper which he has read to us to-night. I should prefer to imitate the modesty of Mr. Sennett, and not to touch on anything I have to say upon vessels of the Navy, of which I know very little, except by hearsay; but with regard to the mercantile marine, I think Mr. Milton has made a very good point in

the remarks he has just concluded, in which he indicates that the line of Mr. Howden's enquiry is different altogether from the line which is necessarily followed in the Navy. The line of Mr. Howden's enquiry is in the direction of the economy of fuel, within certain limits and conditions; the line followed by the Navy is necessarily the means of obtaining very great power in a very short time at very long intervals. Mr. Howden makes a remark in his Paper, which for my own information I should like by-and-bye if Mr. Sennett would answer, and that is, whether in war vessels on a foreign station, fitted with forced draught, this forced draught system has been put in use after twelve months' work under ordinary natural draught, and what is found to be the effect of that change? Mr. Howden has raised the question in his Paper, and I think it would be interesting to the meeting if it is answered. From my experience and knowledge of the Mercantile Marine, I am strongly inclined to think that we must look to some form or another of a closed ash pit in contradistinction to a closed stokehold; I believe it would be found to be simpler, and generally more acceptable, and, what is more important still (if a suitable application of this system can be found), to be cheaper. I think either the Ferrando system, which was alluded to by our President yesterday in his Opening Address, or some such arrangement as Mr. Howden's, will be the one that will be found hereafter to be most applicable for use in the Mercantile Marine; and speaking in relation to those two systems, it does seem to me that Mr. Howden, in his arrangement of dividing the air current and giving unequal power on one side of the bars to the other, is in the right direction, for this reason, that he thereby removes the place of the most rapid combustion from the neighbourhood of the fire-bar to the neighbourhood of the surface of the fuel. That seems to me to be an important point which Mr. Howden brings out in his Paper, though perhaps other people may think differently. Then there is another point. Mr. Howden speaks in general terms throughout his Paper about a moderate water pressure under the bars and a greater pressure above the fire-bars. I looked in vain through his Paper for any indication of what that water pressure was. Speaking from recollection (and I was on board the "New York City" on her first voyage down the Clyde), I think it was from $\frac{5}{8}$ to $\frac{3}{4}$ of an inch, which is of course, comparatively speaking, very small indeed, but it is not mentioned, so far as I can ascertain in the Paper, and perhaps Mr. Howden in his reply later on would indicate not only what is the pressure in the one, but what is the difference between the pressure under the bars and the pressure above the bars.

Mr. W. Watson: The City of Dublin mail steamers between Kingstown and Holyhead, have been referred to both by Mr. Sennett and Mr. Howden, so I am glad to have the opportunity, as being connected with that Company, of saying a few words about what we have done in relation to artificial draught. In the year 1883, just at the time Mr. Butler read a Paper here on the subject, we were arranging to tender for a new contract for the mail service to the Post Office, and we sent in two alternative tenders, one to improve our existing steamers, and the other to build much more powerful and larger vessels. The tender for improving the existing steamers was based on what Mr. Butler had shown could be done by artificial draught in the "Satellite" and the "Conqueror," and I am glad to be able to say that the results we have obtained have fully borne out our anticipations of what would be effected by the use of it. The system we have adopted, I may say, is exactly the same as Mr. Sennett has described in his Paper "on closed stokeholds." Our engines formerly indicated

about 3,000 horse power. We knew that if we could indicate about 20 per cent. more, without extra pressing, we should be able to meet the terms that the Post Office indicated as what would be accepted for the improved mail service. We improved our boilers and made some alterations in the wheels of the vessels, to enable us to use more effectively the extra steam to be obtained by the use of artificial draught. It may here be well to describe the vessels to which we applied this system, they are the large paddle-wheel vessels, which may be known to some here as having carried the mails for the last twenty-five years between Kingstown and Holyhead. They are fitted with oscillating engines of a low pressure jet condensing type, and they indicated, previous to the alterations, an average of about 3,200 horse power, and since we have applied this system of forced draught, they now indicate from 3,800 to 4,000 horse power on the average. The system we adopted was, as I have already said, the Admiralty system of closed stokeholds. We have four stokeholds with six furnaces in each, and we have a double pair of fans on deck over each stokehold, driven by a pair of auxiliary engines, supplied by steam from the main boilers. The fans are six feet in diameter and working as we ordinarily do to indicate the horse power I have named, we use an air pressure of from three-eighths to three-fourths of an inch of water. We have on our trials since we adopted the system worked with as much as $1\frac{1}{2}$ or 2 inches with a proportionate increase of horse power; but the object we had in applying the system was amply gained by the use of, say, on an average $\frac{1}{2}$ inch water pressure. There are two points which have been referred to, which I am able, from the experience we have had, to say something upon. The first is a very important point, *i.e.*, what the effect of the use of these closed stokeholds has upon the ordinary working *quoad* the men. We find the greatest possible advantage is effected by it—the men are much more comfortable and prefer it in every way. This is especially true in bad weather where formerly a great quantity of water would go down through the open deck gratings, and either the inconvenience of the water would have to be borne, or the gratings would have to be covered over with tarpaulings, and the men would have to suffer the inconvenience of want of sufficient air. In the present state of things we are quite as well off in the worst weather as we are in the finest. The second point is even more important, and is, what is the effect upon our boilers of the artificial draught? We cannot find that any detrimental action is to be traced to the new system. The boilers appear to be standing quite as well and giving as little trouble in maintenance as the former boilers did with natural draught since the application of artificial draught to our existing vessels. We have built a new vessel called the "Ireland." We contracted that she should indicate 5,000 horse power under natural draught, and 6,000 under a forced draught, or artificial, as we call it—forced being hardly a correct word to use with reference to so low a pressure. She indicated 6,700 with the artificial draught, and more than 5,000 with the natural draught, and the difference in the speed of the vessel in a 56-knot run between Kingstown and Holyhead was $18\frac{1}{2}$ knots with natural draught and about $20\frac{1}{2}$ with artificial draught. We do not say that we have any right to claim to ourselves credit in the matter. It is more the system that deserves the credit than we do, but at the same time what we have done seems to me to be an interesting and remarkable instance of the success of this effort of being independent of weather, in providing air for the furnaces of our boilers.

Mr. E. N. Henwood: I should like to ask Mr. Sennett this question, What is the amount of evaporation obtained per pound of fuel under the forced draught and under the natural draught? and I should like to ask the same question of Mr. Howden.

Mr. F. C. Marshall: My Lord, I stand in a position somewhere between the two parties represented in this discussion, having had, perhaps, more experience than most people in forced draught of the one sort so ably set forth by Mr. Sennett, and having to do with commercial work, on the other hand, to which Mr. Howden calls attention, and I am sure I speak the sentiments of all present, when I give expression to the deep sense of indebtedness that I feel, and I believe we all feel, towards Mr. Sennett and the officials of the Admiralty, for the very complete—I may say perfect—Paper they have given us as to the result of the working of a great many of the vessels of the Royal Navy under forced draughts, with closed stokeholds. I think we owe them a debt of gratitude. We also are under great obligation to Mr. Howden, for having brought this matter forward so far as he has done. He has done it at great expense and sacrifice to himself; and I think we may say, whether we think it a great advance or not, or whatever we may think of it in relation to what has been done in the Navy, it certainly is, so far as the Commercial Marine is concerned, a step in advance. I think we may fairly say, as Mr. Boyd has already said, that between the two systems of Ferrando and Mr Howden, we may get certainly a very admirable under-grate system; and, under other circumstances, perhaps the closed stokehold system will be the best. So far as the economical results are concerned in the two systems, it has fallen to my lot to carry out some experiments on both systems. I have been making recently, a large number of boilers for the Italian Navy, of the locomotive type, to work at 120 lbs. pressure, and as they have to develope a very high power, I was anxious to ascertain whether these boilers would really develope the power before they were sent out to Italy. I fitted a closed stokehold to two of them in the works, and placed them in the same positions, and as nearly as practicable the same conditions as they would be when on board ship; arrangements were made whereby the water evaporated and coal consumed, the air pressures and quantity passing through the fans, the temperatures of escaping gases, and other particulars recorded in the Table subjoined, could be carefully ascertained and noted. The experiments were made with the object of ascertaining the maximum evaporation obtainable under moderate forced draught pressures, such as would be readily available under ordinary conditions at sea, for the class of vessel for which the boilers are intended, viz., torpedo cruisers. In these experiments I was anxious to ascertain also the respective merits of two kinds of fire-bars. The results were the following:—

[TABLE.

Summary Results of Evaporative Tests with Two Boilers of a Modified Locomotive Form.

Number of Trial.	Date.	Duration of Trial.	Surfaces.		Mean Steam Pressure.	Water.		Water Evaporated from and at 212 Degrees.				Coals.			Air Pressure.			Revolutions of Fans.		Temperature.				Class of Coals.	Remarks.
			Heating.	Grate.		Total Pounds.	Per Hour.	Per Hour.	Per Pound of Coal.	Per Square Ft. of Grate.	Per Square Ft. of Heating Surface.	Total Pounds.	Per Hour.	Per Square Ft. of Grate.	In Stokehole.	In Ashpit.	In Uptake.	Port.	Starboard.	Stokehold.	Uptake.	Atmosphere.	Feed.		
		H. M.					Lbs.																		
1	4 Mar.	1 32	2,232	52·5	125·5	44,969	29,336	35,937	6·969	685	16·1	7,907	5,165	98·3	3·54	3·13	0·65	1,283	1,150	75	1,150	44	37	Cowpen Coal Company's ordinary best steam coals, not hand picked, and rather damp and small.	Ordinary fire-bars, $\frac{5}{8}$ in. thick, $\frac{1}{2}$ in. air spaces, no opening through fire-doors. Both fans discharging into stokehold.
2	13 ,,	1 23	2,232	52·5	125	44,004	31,809	38,998	6·889	742·8	17·47	7,840	5,667	107·9	3·605	3·446	0·25	877	909	66	1,163	37	36	Cowpen Coal Company's ordinary best, as above.	Thin fire-bars, $\frac{3}{8}$ in. thick, $\frac{1}{8}$ in. air spaces, no opening through fire-doors, fires thinner Both fans discharging into stokehold.
3	16 ,,	1 13	2,232	52·5	123	41,816	34,369	41,999	6·62	799·9	18·81	7,720	6,344	120·8	4·955	4·955	0·91	1,244	1,208	66	1,159	38	40	Cowpen Coal Company's ordinary best, as above.	Fire-bars same as No. 2. Both fans on to stokehold, one boiler steaming. Results calculated for two boilers.
4	18 ,,	1 5	2,232	52·5	120	36,264	33,474	40,905	6·566	779·1	18·32	6,720	6,203	118·1	3·07	3·36	0·633	925	1,171	77·5	1,210	40	39	Cowpen Coal Company's ordinary best, as above.	Fire-bars same as No. 2. One fan on to ashpit, other to stokehold. Air getting into fire-doors, one boiler steaming. Results calculated for two boilers.
5	20 ,,	1 41	2,232	52·5	116·2	54,900	32,613	39,821	8·566	758·5	17·84	7,826	4,648·8	88·5	2·0	3·706	0·812	769·2	1,212	87·6	1,200	50	40	Nixon Navigation, new, dry, and good.	Fire-bars same as No. 2. One fan on to ashpit, other to stokehold. No air got through fire-doors, one boiler steaming. Results calculated for two boilers.

In test No. 1 ordinary fire-bars were used, $\frac{5}{8}$ inch thick, $\frac{1}{2}$ inch air spaces between the bars being placed as usual longitudinally in the furnace.
In tests Nos. 2, 3, 4, and 5 thin fire-bars were used, $\frac{3}{8}$ inch thick, and barely $\frac{1}{8}$ inch air space between. The bars were placed across the furnace.
In tests Nos. 1, 2, and 3 both fans discharged as usual into the stoke-hold, but in No. 3 one boiler only was under steam, both fans being used for this boiler.
In Nos. 4 and 5 one fan discharged direct by means of a casing into the ashpit, while the other discharged into the stokehold, so as to prevent the flame from issuing from the fire door when fresh coal was being supplied.
With the ordinary fire-bars used in test No. 1 a water trough was required in the ashpit, as the heat was intense.
With the thin fire-bars used in the other tests the ashpit was quite cool, and the edges of these bars, after thirty hours' steaming, were quite sharp as from the mould.

In the first instance, the boilers were worked under the closed stokehold system; the fire-bars used were of the ordinary description, placed longitudinally to the direction of the draught, as is usually done; they were of wrought iron, five-eighths to three-quarters inch wide, with air spaces, about half-inch. After a series of experiments, these bars were removed, and the furnaces fitted with what, for the sake of distinction, we will call the Ferrando fire-bar, but which is really only an extremely thin bar of cast-iron, about 8 millimetres wide at top, and 4 millimetres thick at bottom, 3 inches deep, placed in short lengths *transversely* to the direction of the draught, as closely together as the roughness of the castings will admit, leaving spaces of from 2 to 3 millimetres in width for the air to be forced through, as will be noticed, in very finely divided streams, and, from the thinness of the bars, bringing it into intimate contact with the fuel in the bars. Our temporary stokehold was fitted with two 3 feet Brotherhood fans. In the first set of trials—with ordinary fire-bars, as also the first of those with the Ferrando bars—both fans discharged their air into the closed stokehold in the ordinary way, and both boilers were worked. In the second set of trials with Ferrando bars, one boiler only was used, and one fan connected directly by a channel to the ashpit of that boiler, while the other supplied air to the stokehold, in order to maintain a pressure in it, to prevent the flashing back of the flame at the fire-door. (The results of the working of this boiler are doubled, so as to compare with the other trials.) The accurate results of these trials are given in the table referred to above, but they may be roughly stated as follows:—The mean weight of water evaporated with the ordinary bars under closed stokehold conditions may be taken at 35,000 pounds, the pressure of air being $3\frac{1}{4}$ inches, the coals burnt per square foot of grate 98·3 pounds. The Ferrando bars, under the same closed stokehold conditions, gave 38,998 pounds of water, with an air pressure of $3\frac{1}{2}$ inches, and a consumption on the grate of 107·9 pounds. Then we come to the same Ferrando bars with the under-grate system—that is the fan connected directly to the ashpit—a pressure being maintained in the stokehold only to prevent flame flashing back through fire-door owing to pressure in furnace, which gives in one experiment 40,905 pounds of water for 3·36 inches air pressure under the grate, and 3·07 inches in the stokehold, the coal burnt being 118·1 pounds per square foot of grate; and in the next experiment 39,821, or say 40,000, pounds of water with 3·7 inches pressure in the ashpit and 2 inches in the stokehold—the coal burnt being 88·5 pounds of hand-picked Nixon's navigation coal in this case, as against the best unscreened Cowpen coal in the former cases. The last named circumstance, which arose from inadvertence, renders the comparison not quite so complete as we could wish, so we must leave it practically out of the question, excepting that it gives us an idea of the fire grate, fire and tube resistance, which may be taken at the difference between the ashpit and stokehold pressures, because the latter was regulated until it just overcame the tendency of the flame to rush out at the open fire-door; in this respect it is valuable, otherwise it must be thrown out. It will thus be seen that in the two previous experiments, allowing for errors in observation and the differences in pressure, the two systems gave in practical efficiency nearly the same results, and that the water evaporated follows the relative pressure of air used.

A Member: What was the quantity of coal burnt per square foot per hour?

Mr. Marshall: In the first of those 118, that is where we had a higher pressure in the stokehold. You will find I say from these experiments, so far as they go or are worth, that whether you have

an open stokehold working under a considerable pressure, and the air going in through the fire-doors, or you have a closed ashpit and the air going under the ashpit, the difference is not very important. I am disposed to give some value to Mr. Howden's view with reference to the injury done by the inrush of cold air on the opening of the fire-doors in the closed stokehold system. I am quite aware of what Mr. Sennett will say, and I know what my own experience is, that we do not suffer in our boilers, so far as our experience goes, from the opening of the fire-doors. So far, we have had no difficulty with our tubes. The system has been applied principally to boilers of the long description having the long flame box in which the air gets heated before it comes in contact with the surface of the plates against which it may come. I quite agree with Mr. Boyd, the time has come when this question has to be faced by the Commercial Marine, and it is a question as to which of the two systems will be the one to be adopted. I think a great deal is to be said for Mr. Milton's view, that to some extent it will depend on circumstances; where you have one or two boilers at the utmost, probably the ashpit system will be adopted, that is with shallow ships, with greater advantages and facilities than the closed stokehold. My own feeling is, I think, seeing that the effect is very much the same in both cases, the tendency would be to go to the closed ashpit—and leave all free above as at present, notwithstanding the advantage which our friend from the City of Dublin Steam Packet Company mentioned with regard to bad weather. There are a great many points that one would like to touch upon. Mr. Wright and those who have preceded me have so exhaustively dealt with the subject, that there remains very little more to be said; but with reference to the Ferrando system with the close bars, which, so far as I know, is a novelty in this country, I will tell you this fact, that in the Harbour of Genoa I saw a vessel with engines indicating something like 3,000 horse-power being coaled from a Newcastle ship with duff coal—that is, this very large passenger steamer running from Genoa to Calcutta was being coaled with coal hitherto treated as refuse, and worth about half ordinary bunker coal. That is a very important fact. I happened to be in Genoa when that vessel returned; the coals had cost for her voyage scarcely more than one-half what the other coal would have done. The result so far as consumption and the maintenance of steam was concerned had been admirable—nothing could be better. There was a slight saving in the quantity of fuel per day, and the speed of the vessel had been maintained much more uniformly during the whole voyage. This is a great fact, and a great fact in the face of the present condition of things. If we can, by using extremely thin bars placed very close together, and using a forced draught, burn coal of one-half the value, then I say we are doing a very great service to the mercantile marine, and we give due honour and credit to our friends, even if they are not countrymen of our own, but to the people of Italy, who have introduced that system. Another steamer which had usually run at something like nine knots now goes about nine and three-quarters to ten knots steadily, on a consumption of two tons per day less than she had been burning of ordinary coal, on duff coal from Newcastle. So it seems to me there is something to be learnt in reference to the use of thin bars, as well as whether apart from or in conjunction with forced draught.

Mr. W. Parker: Sir Nathaniel and Gentlemen, I would like to make a few observations on this Paper. We all, I am sure, feel very much indebted to Mr. Sennett for his able Paper, and the amount of information he has given to us as to the practice in the Royal Navy. Every one I think

will agree that as regards the Mercantile Marine, any further reduction in the consumption of fuel in our steamships must be looked for, as Mr. Boyd has indicated, in the making of the steam and not in the manner in which the steam is used. Of course everybody here knows that at present we have got almost to the end of our tether in the upward range of pressures with the present form of boiler, and we have also an engine capable of utilizing that pressure, and giving us the full advantage of it. Therefore if we look for further decreased consumption we must look in the direction indicated by these Papers, and, as I said before, any information in this direction which can throw light on the subject cannot fail to be most valuable. I think our friend Mr. Howden, whose experience in this matter I have followed with the greatest interest, and the value of whose labours I have always been glad to recognize, seems a little too sanguine as to the results he obtains by this process of his. As I remarked on the last occasion when he read a Paper here, it cannot be held that simply because he reduces the grate surface of a boiler he also decreases the other proportions of the boiler in the same ratio. I visited the steamer "New York City," not at Mr. Howden's request, but merely out of curiosity, for the purpose of satisfying myself what that ship was really doing. There is no doubt that Mr. Howden has settled one point that I think is rather an important one in connection with forced draught, and that is that it is quite practicable to burn from 35 to 50 pounds of coal per square foot of grate in a steamer engaged on a long ocean voyage without any damage whatever to the interior or exterior of the boiler. The performance of the "New York City," I think, has placed that beyond dispute. I find she has been running for about 18 months, and running under rather unfavourable conditions; for everyone will admit that the man who is in charge has a great deal to do with the success or non-success of the machinery or apparatus. It has been the misfortune not exactly of Mr. Howden, but of Mr. Scrutton, the manager, to have had three different engineers in that ship, and the last voyage that she made she had a fresh engineer, and she did better work on that voyage than she did before, and not only had she a fresh engineer, but every one of the firemen was new to the ship. They went out on the Christmas Day, not a very lively day for firemen to go to sea in a ship, and still at the same time no accident occurred, and the vessel returned and gave very good results. Mr. Boyd inquired with regard to the pressure of air that was used in the ship, and the information that I have from the engineer who worked this vessel is, that in the stokehold they have an air pressure of about $1\frac{1}{4}$ inches, but in the grate over the fire they have not more than a $\frac{1}{4}$ of an inch of air pressure. It ranged during the last voyage from $\frac{1}{4}$ to $\frac{5}{8}$ from here to Demerara, and that is not an excessive air pressure; taking the indicated horse power at the moderate amount of 600, and the consumption at 9 tons of coal a day, it gives some 24 lbs. of coal per foot of grate, and that again is not excessive. But the engineers explained that they took an hour to clean a fire instead of a quarter of an hour or twenty minutes, and the other two furnaces by means of a little more air pressure would keep the boat steaming at the same rate of speed. If that were the case, and they were doing the work with two furnaces instead of three, they would be burning about 56 lbs. of coal per foot of grate. As Mr. Milton truly said, it does not follow because the grate surface is reduced that the whole boiler would be reduced, and that, because the "City of New York" was running with half the grate surface that she had, the "Oregon" could run with boilers half the size of those with which she was running. Mr. Marshall has given to us some very alarming, perhaps I should rather say astonishing, and peculiar information as to burning 115 and 118 pounds of coal per square foot of grate per hour. My friend who sat beside

me a little while ago considered he was doing a great work in burning 66 pounds, but Mr. Marshall's experience far surpasses his. I will make one remark with regard to the rush of cold air. Mr. Howden gives us something there worth consideration. In the Navy the trials hitherto have only been for six or seven hours, and they have not had much trouble. But in a steamer that is engaged to steam for 30 to 40 days without stopping, it is a matter of very great importance. For instance, I have in mind a steamer that has done well until her last voyage—and she is now on her tenth voyage round the world—has in fact never had a mishap, but on the last voyage she had to steam from Australia round Cape Horn to London, a thing which would not under ordinary circumstances be attempted by a steamer of that class. In consequence of bad weather she had to be driven at the top of her speed, and had to steam with bully coal taken from Australia. This bully coal has about 50 per cent. of ash in it. It is of such a dirty nature that the fires require to be cleaned twice as often as with the ordinary coal. The consequence was that these doors had to be opened, and the fires cleaned every watch; there were twelve furnaces in the ship and they had to be cleaned every watch. When that ship arrived in this country we were very much astonished to find her furnaces had collapsed, but when we came to inquire into the matter and ascertained the cause of the collapse we were not so much surprised. The cold air coming down that stokehold and rushing down those furnaces caused a creeping to take place in the furnaces, and at each creep there was thrown off a scale, till a large accumulation of scale took place, and the end of it was, that the furnaces became hot and the pressure crushed them in. The idea I formed was confirmed by the fact that each furnace that collapsed was directly under the ventilator. Of course they had always been under the ventilator before, but they had not been opened so frequently, and the inrush of air was only half on the previous voyages, but on this voyage it was so excessive that the cold air caused the contraction which Mr. Howden has spoken about which is so detrimental to boilers.

The Chairman: It is impossible to exaggerate the value of the discussion we have had, but in justice to the Paper which is to follow it, we must bring it to a close. There is another subject to be taken, and we must give to Mr. Sennett and Mr. Howden the opportunity of replying, and I think if they will be good enough to confine their remarks as much as they can to what is necessary to be said, we shall get through our business satisfactorily.

Mr. R. Sennett: Mr. Chairman and Gentlemen, the subject has been so fully discussed by those who have done Mr. Howden and myself the honour to make remarks on our Papers, that I think there is very little to be said by way of reply, as the different speakers have to a certain extent replied to each other. Mr. Wright compared the dimensions of the boiler of the "New York City" with those of boilers working with natural draught and in closed stokeholds, and showed that the boiler in that ship was not, strictly speaking, worked with a forced draught system. Mr. Milton called attention to that, and said that in the Mercantile Marine and in the Royal Navy we were necessarily working in opposite directions; that in the Mercantile Marine the great object was to secure economy of fuel without reference to the capacity and weight of the boiler, whereas in the Royal Navy the principal object was to gain power with reduced weight of boilers without so much reference to economy of fuel. Mr. Howden does not seem to take quite the same view as Mr. Milton does. I take it the main object of his Paper is to prove that if his system had been applied to the "Oregon," instead of nine boilers of

a certain size, the same power might have been developed by six boilers of a considerably smaller size. I think this shows that Mr. Howden himself believes that his system would be attended with a reduction of weight and space required for the boilers. But taking the "New York City" as a basis for argument, I find the six boilers that Mr. Howden proposes to put in the "Oregon" would be equal to rather less than 8,000 horse power, instead of the 12,000 I.H.P. stated. Mr. Boyd asked if any of the war vessels fitted with closed stokeholds have been employed on foreign stations. Not many of them have as yet, but the "Satellite" referred to by Mr. Butler in his Paper three years ago, has just completed one commission in the Pacific, and is now going to be re-commissioned in China, and, so far as we know, everything has been perfectly satisfactory. The officers of the ship have had no restriction placed on them as regards the use of the fans and forced draught. There are also several other vessels that have been away for many months. In accordance with the Admiralty regulations full power trials have to be made at least twice a year, and so far, we have had no complaint whatever as to any injurious effect on the boilers by working the fans. I do not think that I need say much about the question of the cooling effect of the air on the boiler. That is a circumstance that must occur with all boilers except, perhaps, those with closed ashpits. Nearly all steamships at the present time are fitted with ordinary boilers, and when the fire doors are opened air must get in over the fires, but we find, as a matter of actual experience, that the boilers do not suffer in the serious manner stated, except in very exceptional cases. The case quoted by Mr. Parker, as he admits, is an exceptional one, and occurred under very exceptional circumstances.

Mr. Parker: No, it is not.

Mr. Sennett: You have only quoted one example out of all the cases in the mercantile marine.

Mr. Parker: May I offer a word of explanation to set that right. It is a fact that as to the Allan steamers, trading between Liverpool and Quebec, instructions based upon the experience of the Engineer-in-chief have been issued, to the effect that when they arrive at the other side in winter time they must, under no consideration, open the furnace doors, but allow the fires to burn out, because it was found if they did open the doors, that the cold air would rush into the furnace and give them a great deal of trouble.

Mr. A. C. Kirk: Might I be allowed to explain that where the scale fell down thickly in these boilers was off the bottom of the corrugations.

Mr. Parker: I did not say that it fell down.

Mr. Sennett: All I wish to explain is, that this question of the cold air getting into the furnaces exists with regard to nearly every boiler, and is not peculiar to the closed stokehold system. In my opinion the evil effects supposed to be produced by air entering the furnace through the open fire-door are much exaggerated, and as a rule they exist more in imagination than in actual practice. Mr. Henwood asked a question about the evaporation of one pound of fuel with forced and natural draught. I am unable to give him information on this point, as we have made no special evaporative

experiments. I think I have nothing more to say, except to thank you for the kind way in which you have received my Paper.

Mr. J. Howden: I find myself in this discussion, and very unwillingly on my part, brought as it were into antagonism with the gentlemen of the engineering department of the Admiralty. I can truly say it has been far from any desire of mine to feel myself placed in opposition to gentlemen whom I highly esteem. The question of the comparative merits of working on the system of forced draught I have described, and that of the closed stokehold system has, however, arisen, and I felt bound to state the facts within my knowledge of the destructive effects of the rush of cold air into a boiler when working at sea after having been subjected to a high heat such as must occur in boilers worked on the closed stokehold system. It should, at the same time, be remembered that when the Admiralty began to apply the closed stokehold system of forced draught to the steam-ships of the navy, there was no other system before them. Though not a design of their own, they adopted it, and have applied it to a great many steamers, and it is but natural that they should feel some tenderness towards their own nursling. Nevertheless, I have only stated what I firmly believe to be correct in regard to the relative effects of these systems of forced combustion on boilers when worked at sea, in the ordinary way, with salt water, and at rates of combustion considerably higher than are attainable by natural draught. There have been some remarks made regarding the comparative power, taken from the boiler of the "New York City," viewed in relation to its size. As I explained in my Paper, I had to design this boiler to work, not only by forced combustion, but by natural draught also, and I had to sacrifice a good many advantages which I could have secured had the boiler been made for forced combustion only. The diameter of the boiler, as I have already pointed out, could have been reduced considerably without affecting the heating surface, but as there was abundance of room for the boiler, it was made large and with very wide water spaces, as the plans show. I could also have reduced the size of tube considerably and have had more effective heating power with forced draught, with less surface. It is, therefore, quite out of place to compare this boiler of the "New York City," on the basis of its capacity, as Mr. Wright has done, with those boilers of the Navy, where with a large grate surface and irrespective of fuel, a large power was forced out of them for a few hours under the closed stokehold system. If Mr. Wright had given the quantity of coal used to produce the respective horse powers, then a proper comparison could have been made. As I have said in my Paper, economy in fuel is the foundation of my system, and it has been with the object to ensure economy that I have adopted the proportions which exist in this boiler. It was not required to produce a very high power in this boiler, and, therefore, I used a grate proportioned to the power wanted with easy working at sea. For mercantile steamers, take the example of the "Oregon," given in my Paper, for reduction in size of boilers on my system to produce an equal power. I there show six smaller boilers, substituted for nine larger ones: 36 furnaces for 72 and an aggregate fire-grate of 641 sq. ft. for 1512. This was considered an impossible reduction when I read my Paper on the subject in 1884, but I have shown that I have accomplished, proportionately, more than this in the "New York City," in ordinary work at sea without any exertion whatever. This being so, it is clear that I could reduce the boilers considerably more in proportion than I have proposed in regard to the "Oregon." You have heard how that with two furnaces only and 24 sq. ft. of grate, the "New York

City" has, when tried, not only been able to drive the engines at 630 I.H.P., but also at the same time to raise the steam from 80 to 85 lbs. pressure and blow off at the safety valves. If the latent power in a system of combustion that can accomplish such a feat be considered for a moment, there will be a very different conclusion arrived at, than that which has suggested the question, whether or not is this a forced draught system? What I say in reply to this is, that there is no forced draught system, closed stokehold, or any other that could dare attempt at sea such a production of power per square foot of grate with salt water in the boilers, for, as I have mentioned, the "New York City" has virtually been worked with salt water only ever since she left the Clyde on 13th October, 1884. There have been questions asked by some speakers as to the air pressure used in the furnaces of the "New York City." I cannot tell of course very well what pressures may be used by those on board when at sea; I have no control of the men on board, but I may mention the pressures that should be worked at, to produce about 20 I.H.P. per sq. ft. of fire grate, in such a boiler as that of the "New York City." With the fire-bars, as presently used in that steamer, about three-eighths of an inch of water pressure would be required under the fire-bars, and with a proper area and size of holes in the air distributing boxes above the fuel, about 1 inch water pressure there. As I have endeavoured to explain, the difference of increasing the velocity of the air entering above the furnace, say from 20 to 50 feet per second, which I can do quite easily by means of the regulating valves, is very marked on the power of the furnace. The force at which the air strikes the fuel is as the square of the velocity, so that at the highest velocity the force is as 25 to 4 of the less velocity. By this means, therefore, I can perform a rate of combustion quite unattainable by other conditions, as a much reduced weight of air entering above has an equal or greater effect in producing perfect combustion, while as it scarcely adds to the pressure of air in the furnace above the fuel, it does not hinder the passage of the greater amount of air at the lower pressure from below ascending through the fuel and producing a high rate of combustion. It is by the balance of these differential pressures of air above and below the fuel that the power and economy of these furnaces on my system are produced. By the regulation of these almost any power can be produced at pleasure. There have been a great many points referred to in the discussion which I would have wished to notice had time permitted, but there is only one which I will now detain you with. It is that of the closed ash pit system which has been referred to by some speakers to-night as the "Ferrando" system. That is exactly the system I tried in 1862 and which I described in my Paper of 1884. I worked that system all round with different pressures under the grate-bars, and as I have described, whenever I increased the combustion above natural draught rates the troubles increased in all directions. It is not a system fitted for a high combustion if economy and workability are desired. The manner of working described by Mr. Marshall, with thin fire-bars, as used by Mr. Ferrando, as I understand him, is a combination of the closed stokehold and the closed ashpit systems, one fan as mentioned, blowing into the ashpit of the boiler in which the experiments were tried, and the other fan into the closed stokehold built round the boiler, giving air and maintaining pressure above the bars. By this means, which is after the manner of my system in effect, I have no doubt a high combustion will be attained. As the time is now up, I will conclude by thanking you, Gentlemen, for the great attention you have given to my Paper and to this discussion.

ON MODERN IMPROVEMENTS IN THE WORKING OF CABLES AND THE STOWAGE OF ANCHORS.

By Samuel Baxter, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 15th, 1886 ; Sir Nathaniel Barnaby, K.C.B., in the Chair.]

The efficient operation of the various appliances by means of which the anchors and cables of a ship are worked, is a matter of so much importance, and these appliances have undergone so much improvement during recent years, that I have readily responded to a suggestion made to me by one of your Vice-Presidents by preparing the present Paper for the acceptance of this Institution.

Having been personally devoted to the development of these appliances since 1850, and desiring to give to my observations as practical a character as possible, I propose to direct attention to the improvements in windlass and capstan gear which have been brought about since that year. If it should be found that in reviewing these improvements I have to make occasional, and even frequent reference to my own labours in this department of naval work, I trust that I may be excused for doing so, on the ground that I have been almost wholly engaged in it for so lengthened a period.

The first improvement to which I invite attention was introduced in 1851, for facilitating the working of chain cables upon windlasses with wood-barrels. Previous to that date it was compulsory to range the cable on the fore side of windlass before letting go the anchor in merchant vessels, and when purchasing anchor it was necessary to stopper the cable on the fore side of the windlass for shifting back the turns of the cable as it was hove in, in order to prevent the mounting of the incoming parts. This caused considerable delay, as the operation had to be performed repeatedly in purchasing the anchor. The plan then introduced (by myself) was that of fluting the barrel, whereby continuous heaving was provided for. The time employed in purchasing the anchor was thus greatly shortened, and the plan was consequently extensively adopted.

With ships of war it was the practice to purchase anchor by a messenger chain worked on a sprocket wheel fitted to the capstan, the main cable being attached to the messenger. This arrangement involved considerable delay by preventing continuous heaving, frequent stoppers being necessary for the attachment and re-attachment of the main cable to the messenger during the purchasing.

Some years later it became the practice in ships of war to use a cable-holder fitted at the foot of the capstan, by which the cable was continuously hove in. In consequence, however, of the chain cables manufactured to Admiralty rule being made with a shackle and a swivel alternately in every length of 12 fathoms, repeated reports were received complaining of the shackles and swivels slipping and surging, and sometimes of their leaving the cable-holders. Admiral Sir Spencer Robinson, when Controller of the Navy, gave instructions for some experiments to be made for the purpose of proving whether the swivels could be dispensed with. It was found that on a moderate tensile strain being applied to a length of cable with a swivel in it, mid-distant from the ends, that two men with an iron lever could not turn the swivel, and it was then decided to make an order for the swivels to be taken out of all cables, leaving only one at the outer end, at such a distance from the anchor as to prevent it coming upon the capstan cable-holder, and one in the inboard end in case of having to reverse the cable.

After this, in order to bring the working of the shackles as well as the swivels under careful observation, it was made a standing order at H.M. Dockyards that all ships having cable-holder capstans, as they came on for commission, should drop anchors, veer out 100 fathoms of cable to each anchor, and then heave in, making certain that the shackles were brought on to the cable-holders, some with their pins vertically and others horizontally, under strains such as they would be liable to in actual service. A report was in each case forwarded to the Admiralty, signed by the officers of the yard present and a representative from the contractors. These trials were witnessed by me for several years, and afforded the opportunity for much practical observation, which resulted in the shaping of the stops of the cable-holder to a form that could be depended upon for working the shackles in both positions without slip or surge, and, provided that the shackles with the end and enlarged links are made to the rules now issued by the Admiralty to chain cable contractors, all the previous difficulties are obviated.

The application of steam to the working of capstans materially shortened the time in purchasing, but as the cables in "letting go" anchor had to be around a riding bitt, and in

purchasing removed from the bitt and placed around the capstan cable-holder, a number of men were required with chain hooks to drag the slack chain to the deck pipe, as it was hove in, for paying into the locker. This disadvantage after a time gave rise to the cable-holders being mounted in the form of a windlass over the chain locker. By this means, each cable was allowed to remain in position both for "letting go" and "purchasing," and the men and the use of the chain hooks were dispensed with. This plan was more prominently brought forward in consequence of H. M. S. *Northumberland* parting her cables under circumstances which induced the Admiralty to make a searching enquiry as to the cause. It was considered that the primary cause was to be found in the method then in use of "bringing to" by the service lever compressor, which caused the cables to be weakened by the sudden shocks that took place in bowsing to, and it is not unlikely that many cables have been parted from the same cause.

An experiment was then made on board H. M. S. *Newcastle* with a cable-holder over the chain locker, fitted with a frictional brake, for the purpose of proving that a vessel could be "brought to" by it with greater safety and precision than with the service compressor. This proving satisfactory, a similar one was fitted in H. M. S. *Warrior*, and, favourable reports being received of the advantages obtained, the experiment resulted, I believe, in the new ships being furnished with this plan, worked by steam in addition to the capstan as hitherto.

I may here remark that since cable-holders have been adopted in windlasses many methods have been used for connecting them to the working spindles for purchasing and releasing them for "letting go." Nearly all such connections have been rigid, and therefore difficult to release. Now, it is very certain that a connector capable at the same time of resisting the required strain, and of yielding should a dangerous strain occur (thereby preserving the cable) and so arranged as to be in the power of one man to release it, would be the most efficient. Such we have in the compound frictional connector, which consists of a series of plates alternately connected by feathers to the cable-holder and to the spindle, which plates are readily compressed for resisting any required strain, and instantly released in any position, with the advantage of acting as a continuous spring in riding, and without the disadvantages of an ordinary spring, the action of which is limited, and below its limit takes effect in the sawing of the hawse pipe.

When this plan of employing compressor plates was first used, the series consisted of wood and iron plates alternately, which proved unsatisfactory in practice. The difficulty of

using an entire series of iron plates was, that when set up, they could not be readily released.

From experiments made in a windlass at Lloyd's Testing House, Poplar, I found that it required three tons to release them when they had been set up to resist a strain of forty tons.

It, however, occurred to me that this difficulty might be removed by putting a permanent bend in each alternate plate. This I tried, and it had the desired effect, and from that time the use of iron or steel plates alone has been preferred.

Improvements were continued in the horizontal windlasses: In the case of H. M. S. *Shannon*, in 1877, I was present when an accident happened, by the connector lever not being withdrawn, and the engine unexpectedly started. The lever in consequence was brought in contact with the iron coaming, setting the frictional plates fast before it was sheared off, causing considerable delay in attempting to release them owing to the difficulty in getting at them. In consequence of this I considered it would be necessary to devise a plan to prevent similar omissions becoming disastrous, and close consideration of the subject suggested a readier means of dismounting the cable-holders, and of thus getting at the frictional plates.

It may be well to point out that, in order to get at the frictional plates in horizontal windlasses in ships of war, it is necessary to lift the whole windlass, and that, in consequence of the cables being taken over the top and paying down into the locker, a guide has to be fitted to keep the cable sufficiently around the cable-holder to prevent it slipping in heaving. This guide is an obstruction in "letting go" anchor, and if it were required to mount another cable it could not be done without unshackling, thereby causing considerable delay. For this reason four cable-holders were used for the bower and sheet cables. Now these arrangements appeared capable of improvement, and gave rise to my devising a vertical arrangement, which would give free and ready access to the brake and connector, greater freedom for veering, and enable the two cable-holders to do the work of four, as the bower and sheet cables could be worked upon the same cable-holder. This not only reduced the cost and the weight, but it also enabled one to comply with the conditions laid down by the Admiralty, that, in consequence of the difficulty in veering with the horizontal system, all steam windlasses should be capable of heaving cable outboard by steam and hand as well as of heaving in.

These improvements I embodied in a model which was explained at the Admiralty

and which resulted in their Lordships deciding to have these fittings for the *Admiral* class of ships, and H. M. S's. *Benbow*, *Rodney*, and *Howe* have been so fitted. The gear is made of annealed cast steel throughout, and consists of worm wheels fitted upon the cable-holder spindles, actuated by worms upon horizontal shafts, on which are pinions gearing into the capstan crown wheels, by which the windlass is worked by hand in either direction, the windlass being at all times kept pawled by the worm gearing, to the extent of the frictional connection set up.

I would here endeavour to point out some of the advantages obtained by the system of mounting the cable-holders upon a vertical axis (besides those already mentioned) such as a ready access to the frictional plates, and the means for taking one cable off and bringing another cable to the same holder. By the cylindrical bearing, which is made in one with the base plate, there is obtained a sufficiency of strength equal to a riding bitt, and this admits of dispensing in some ships with one pair of bitts.

Another important point well worthy of consideration, is the excessive cost of the fitting of any horizontal plan in comparison with that of a vertical plan. With the former, openings are required in the decks, which always mean the cutting of beams and the fitting of carlings and other work to make up for the lost strength. With the vertical windlass none of this is needed. The base plate, with its fixed shaft, is secured to the deck, and the spindles for the gearing are passed through to the worm wheels, no bending action taking place with the driving spindles, as there would be were the cable-holders mounted direct upon them. The worm is upon a horizontal shaft, and is run in a lubricating trough, which, to my mind, is the only satisfactory position for a worm to work in. In addition, the whole gear is simplified.

It was found in the ironclad *Riachuelo*, with the vertical arrangement, that it was not necessary to set up the connector for heaving chain outboard, as by the weight of the plates one above the other sufficient friction was created for this purpose. This, it will be seen, is very advantageous in anchoring in shallow water, where it becomes necessary at times to heave a little cable out in consequence of the cable inboard balancing that already out. The moment the anchor takes ground, the cable-holder is free to revolve by the additional pull until it is required to check it, which can be readily done by the brake lever, without stopping the engine until the ship is brought to.

It has been erroneously stated, with reference to the frictional brakes arranged in these vertical cable-holders, that the weight of the plates one upon the other prevents the

cable-holder from revolving freely, and that this is a disadvantage, whereas in practice, as already mentioned, it is of the greatest importance and value. If by any plan they are kept apart, a danger is created, as they must in such case be set up for heaving out; and as it cannot be known when the anchor takes the ground, mischief may arise by the resistance offered to the unexpected shock.

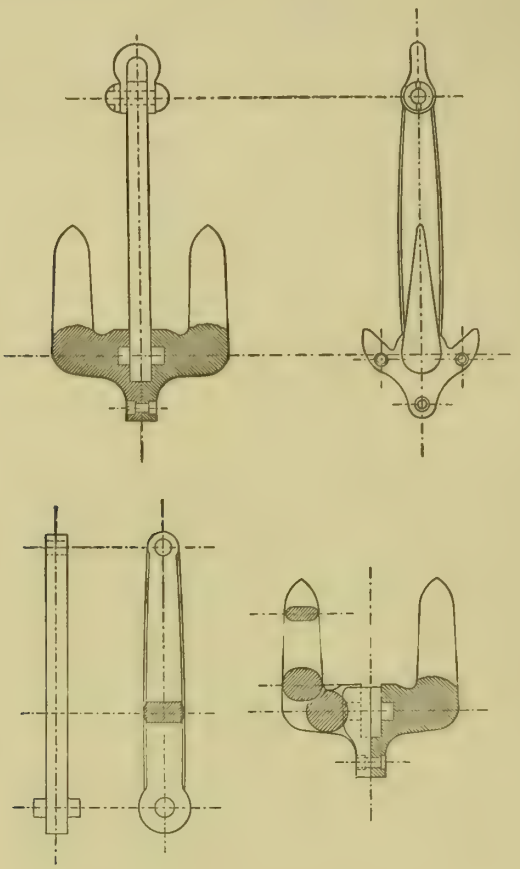
I come now to the stowage of anchors. I had witnessed several accidents in catting, fishing, and stowing anchors during my attendance at the trials of the cable gear, and had frequently thought of some means for obviating the dangerous operations, and after the fatal accident on board the *Royal Alfred*, at Spithead, where an unfortunate blue jacket had his neck broken in securing the shank painter, I devised a plan in 1870 for dispensing with this operation, and proposed it to the Admiralty, but nothing at the time came of it.

The introduction of ram stems brought about the necessity for providing a means of clearing the anchor from them for catting, and the difficulty, I believe, first became apparent in H.M.S. *Helicon*, when with the ordinary means great difficulty was found in clearing the anchor. To remedy this defect the late Staff Commander, J. W. Reed, R.N., proposed (through his brother, now Sir Edward Reed) the adoption of the ground chain and pennant, and the plan was adopted by Sir Spencer Robinson, the then Controller, and answered so well that it soon became general in our own and other services.

In the case of the *Admiral* class of ironclads, the pennant has to be taken direct from the fish davit to the capstan body, which is a very risky operation. I fully believe that it would be found a very much safer plan to use for this purpose a steel wire rope (now that a wheel for working and holding a steel wire rope has proved to be in every way reliable) which would get rid of the jerks and surges that are bound to take place with a chain pennant used direct upon the capstan body.

This brings me again to the plan which I have already mentioned for dispensing with "catting" and "fishing," and with which I did nothing further until 1882, when, having become thoroughly convinced of its practicability by experiments, both with the form of anchor and the formation of the seating, I resolved to patent and introduce the plan. The formation of the seating was such that the anchor could be hove direct into it, by enlarging the mouth or entrance of the hawse pipe to receive the shank head of the anchor, and arranging a recess to receive the flukes, so that the anchor seats itself automatically within

the skin of the ship upon being hove up. The anchor which I patented to suit this object is shown in the accompanying figure.



The reason why our Admiralty have been slow to adopt the plan appears to me to be that naval officers have set their faces against double-fluked anchors, it being feared that they are not at all times to be relied upon; a conclusion arrived at by their experience of the Martin Anchor. Now, any one may see the reason that causes this to fail in certain bottoms. Owing to its great weight, the head of the shank through which the flukes pass buries itself, the arms or flukes are consequently kept out of the ground, and as the anchor is dragged, the arms slide along the ridges of the channel thus formed. The occurrence of this kind of action was, I am informed, proved by a diver being sent down after a vessel had been lying with an Admiralty and a Martin anchor down at Spithead, the diver reporting that the arms of the Martin anchor were in the position I have described.

Now, such action as this cannot take place by any possibility with the anchor which I use for seating within the bow, as the flukes work with the head, the action of which is, that directly a strain is brought upon the anchor lying upon the bottom, the projection of the head tends to cant the flukes, and thereby compels them to enter the ground.

Admiral Azevedo took a favourable view of the plan, and had it fitted in the Brazilian ironclad *Riachuelo*. Some proof of its success, and of the holding properties of the anchor is that the Brazilian Government having since built a second ironclad, the *Aquidaban*, the plan was again adopted in her. The anchors were in the former case 66 cwts. each, and in the latter 60 cwts. each. The whole operation of "letting go," "bringing to," "purchasing," and seating the anchors is done by two or three men, and at the trial of the *Riachuelo* the two best bower anchors were "let go," four shackle lengths of cable were veered out to each, and the anchors were hove up and stowed, all within the space of 40 minutes.

I trust that the foregoing account of the improvements which have been effected during recent years in the apparatus for working anchors and cables on board ships will prove an acceptable contribution to the practical records of this invaluable Institution.

DISCUSSION.

Mr. W. H. Harfield: Mr. Chairman and Gentlemen, having been the inventor of the compound brake which Mr. Baxter has described, I think I have some authority for speaking on the subject. I maintain that its application to a vertical capstan is entirely wrong, because the weight of one series of plates always resting upon the other, prevents that freedom of action which is so necessary when letting go anchor. I am not quite sure whether it occurred when you (Sir Nathaniel Barnaby) were in office, but at all events it is well known that when the "Howe," of the "Admiral" class, came round from Pembroke to Portsmouth, the anchors, of between six and seven tons weight were let go with a drop of about 40 feet of slack cable, and they were brought up by the great friction of the plates, simply resting upon each other, before they touched the bottom. I think the system of brake plates as described in Mr. Baxter's Paper is wrong for *vertical* capstans, because in letting go it is as necessary that there should be absolute freedom as in purchasing that there should be proper connection. With regard to housing the anchor in the hawse-pipe. I think that is not of much value if I am right in stating (and I understand it is a fact) that at the recent Admiralty trial of six different kinds

of anchors at Portsmouth, 'stockless' anchors (which alone can be drawn into a hawse pipe) were proved to be most unreliable and useless. With regard to this part of Mr. Baxter's Paper, I have to-day received a letter from Messrs. McNichols and Co. of Glasgow, who purchased Messrs. Scott and Riddel's patent for housing anchors, taken out in the year 1878, and they desire me to state that at the Inventions Exhibition last year, they were awarded the medal for their arrangement of enlarged hawse-pipe so as to heave a stockless anchor into it. You, Sir Nathaniel, will no doubt have better information than myself as to whether stockless anchors can be relied on, or not, for the ships of the navy. I could take considerable exception to Mr. Baxter's remarks with regard to the action of my compound brake for *horizontal* purposes; but the hour is late and therefore with these few remarks particularly emphasizing the fact, that the brakes *as* described by Mr. Baxter are not applicable to the *vertical* system, I will conclude my observations.

Mr. T. S. Field: As one of the staff of the late firm of Samuda Brothers, I was present at all the trials which took place with the Baxter anchors on board the "Riachuelo" and the "Aquidaban," the second ship that we fitted with these anchors. As regards Mr. Harfield's remarks, I can only say that I know nothing of the fitting of the hawse pipe in the "Howe," and a good deal depends upon the form of the hawse pipe, but in the "Riachuelo" and "Aquidaban," we found that by keeping about three slack links of chain before the manger the anchors always went and took the cable out of the lockers, and I believe that in the Millwall Docks the anchors of the "Riachuelo" were let go about forty times before the ship went into the river—not only for our own satisfaction as builders that we could depend on the anchors during our steam trials, but also to show the Brazilian inspecting officers that the working was in every way to be relied on. I also may mention that when the crew who had not witnessed these trials came over from the "Brazils," they arranged the anchors of their own accord and let them go in the Albert Dock without a hitch. I believe the photograph here is the bow of the "Riachuelo." It shows the great advantage of the stockless anchor in a war ship, enabling, as it does, a forward fire to be obtained *across* the bow. The photograph also shows that we were enabled to stow two sheet anchors of the Admiralty type on the forecastle over the Baxter anchors. I can from my experience say that for quick and reliable working, I have never seen anything to equal the Baxter windlass and anchors as fitted in the "Riachuelo."

Mr. L. Benjamin: I only offer a few remarks in regard to the anchor of Mr. Baxter, and, more especially, as to the seating of the anchor. I think, with regard to merchant vessels, it would be very desirable to introduce such an anchor in more instances than has been done before. I believe in many of our present merchant vessels the forecastle is nothing but a cumbersome relic of bygone times. Many of our forecastles and poops are not what they ought to be, and an anchor like this, and especially an anchor seated in the way Mr. Baxter has introduced, would do very much to alter matters to a state which is more in accordance with what is really required. Instead of the old-fashioned ship-wright's ideas, we ought to have the vessels more in accordance with the views of the modern naval architects, and, I believe, this kind of anchor will effect an improvement in this direction.

The Chairman: I do not see anyone here who is capable of giving any information as to what happened in the "Howe," except Mr. Baxter, who, in reply, will probably give us such information

as he has. He is very likely to know what the facts are, and we may trust him to give them us correctly.

Mr. J. M. H. Taylor: Mr. Chairman and Gentlemen, The question of which is the best anchor is a matter of indifference to me. I had occasion to look round for an anchor which would stow easily and be out of the way at the same time, be handy for letting go and for hauling back into position. It happened at the time that Baxter's patent anchor was illustrated in one of the leading engineering papers, and, as it struck me as being the very thing I was looking for, I made strict enquiries regarding it, and was informed the Baxter anchor worked most satisfactorily as fitted on board the S.S. "Algoma." I had some experience with a similar anchor some years back, which was patented, but this anchor did not stow so conveniently.

Mr. S. Baxter: I have simply to say, with reference to the remarks made by Mr. Harfield, that I cannot agree that he was in the first instance the inventor of the compound frictional brake.

The Chairman: I beg pardon, Mr. Baxter. I think it would be better that the question of priority of invention should be left without being gone into. I think if you will kindly give us the facts concerning the "Howe," and any other facts that may be interesting, it would be better to leave the question of priority of invention alone.

Mr. Baxter: With reference to Her Majesty's ship "Howe" I was on board of her at Pembroke, when the anchor was let go, and purchased, and I can safely say that the anchor took the cable out most freely. When the anchor was down the cable was hove out to some extent by the engine and it was afterwards hove in, and in bringing the anchor to "cat" there was some difficulty in consequence of having to take the chain pennant round the body of the capstan. They had taken no less than five turns round the body, and they had attached to the off coming part of the pennant a watch tackle. The engine was started, and as may be readily understood by most gentlemen present, the watch tackle could not take away the slack chain as fast as the engine hove the capstan round with the pennant, the consequence was that the slack chain gathered on the fore side of the capstan body, and carried away the clearing guide, which caused some hindrance, in fact so much hindrance that the anchor dashed against the side, and in dashing against the side one of the guy eyes of the fish davit gave out and the anchor went back to bow again. Beyond that I know nothing. I only know it was a long and tedious job to get the anchor to deck, but that ultimately we did so, notwithstanding all these impediments. With regard to the hindrance in letting go the anchor by the contact of the plates, I think Mr. Harfield will find if he carefully considers the matter, he is terribly in error, for there is nothing I know of in all my long experience that is equal to the advantages given by the friction plates being upon a vertical shaft. They rest one upon the other, and, as may be expected, they offer a certain amount of frictional resistance sufficiently to heave the slack cable out of the locker. But when the anchor begins to take the ground, the extra strain brought upon the cable overcomes that trifling connection, and the cable holder then revolves freely. In order to bring the ship to, all they have to do is simply to deal with the brake lever. The engine itself may be kept at work the whole of the time, until the ship is brought to; there is no need whatever to stop the engine; the engine may go

on working. When the ship is brought to, and all the cable is out that is needed, the engine may then be stopped, and by an extra pull on the lever all is ready to purchase the anchor at a moment's notice. I am very pleased to find that I have somebody here to assist me. I had no idea that Mr. Field was here, but he having stepped forward and so clearly confirmed my statement as to the "Riachuelo" is very encouraging. Mr. Benjamin, also Mr. Taylor, made observations which I suppose are from a careful investigation of the matter, as well as another gentleman who spoke of the "Algoma." The "Algoma" is a merchant vessel fitted by the enterprise of Mr. Pilcher, the gentleman who had the superintendence of the building of the vessel, and it has proved a complete success; insomuch that before the vessel left Sunderland where she was born, I may say the Committee of Lloyd's happened to be down at Sunderland, and they asked the superintendent if he could manage to allow them to see the letting go of the anchors and the heaving up. He said he should be delighted, and the whole of the Committee accordingly left their quarters, and went up and saw the anchors let go. I am fully satisfied that it cannot be contradicted that the whole Committee expressed themselves thoroughly satisfied with the efficient and ready working of the anchor out and into the seating. I may say the Austro-Hungarian Lloyd's have had these anchors now from the commencement of 1883. They fitted the anchors in the bow of a ship approaching 4,000 tons register. She went on a voyage to Hong Kong and back to Trieste. After that they were building another one, and they ordered the same fittings to be supplied to her. They went on and ordered a third set, and at this present moment I am preparing another set of anchors for a fourth ship approaching 4,000 tons. So taking that and the "Riachuelo" into account, I think it is sufficient to show that the anchors must do their work; besides which the Trinity Corporation of London made a trial of the anchors in 1884; they put one of 10 cwt. on board one of their steam tenders, and directed the captain to carefully examine the action of the anchor. Some months after this they ordered an anchor weighing three times that weight for one of their large ships, viz. 32 cwt. That was in use for some time, and then some months after that they ordered a third, 30 cwt., and since this a fourth of 30 cwt., and at the present time we are making a fifth for them. I don't know that I have anything further to add, and can only thank you for the kind way in which you have received my Paper.

The Chairman: Gentlemen, I have had a great deal of experience of the work which has been done for the Navy by Mr. Harfield and I should not have thought that it needed anything to stimulate him to throw his whole heart and energy, and he has both in abundance, into his work; but it appears he will have that additional stimulus in the future. I wish him and Mr. Baxter much success under the conditions.

INSTITUTION OF NAVAL ARCHITECTS.

FRIDAY, *April* 16*th*, 1886.

(THE RIGHT HON. THE EARL OF RAVENSWORTH, *President, in the Chair.*)

The PRESIDENT: Gentlemen, there is an important matter which I have to bring before you. The Council have decided to-day by an unanimous vote, and the proposal was very cordially received, that this Institute hold an Autumn Meeting this year in connection with and on the occasion of that very important Exhibition which is to be held in Liverpool. We have had some communications, not very lengthy but sufficient to induce us to believe that the Institute would be most hospitably received, as strangers always are received, in Liverpool, because I can speak from experience as to that. I think it would add great interest to that Exhibition, and I am quite sure it will be to the advantage both material and moral, if I may say so, of this Institute, that such a meeting should be held, and I put it to you, Gentlemen. I think it necessary to make this communication to you, and I also ask, in the event of your approving of it, that each man will do his utmost to induce Members of this Institution to read Papers on that occasion. I shall be very glad to hear any observation that any gentleman may wish to make on that subject. It is my duty to make that communication to you, and I hope it may meet with your approval.

(The Resolution having been put to the Meeting was carried unanimously.)

EXPERIENCE IN PROPULSION WITH THREE SCREWS.

By Monsieur M. Marchal, Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

Some months ago the late Director of Naval Construction in England, Sir N. Barnaby, when enumerating the improvements to be effected in the application of propellers to war ships, wrote as follows:—*

"Armour has been largely employed to protect far more than the propelling and steering mechanism in war ships, but it is gradually being brought down even in France, where the system was invented, so as to cover, in the largest ships, only a height of about 3 or 4 ft. out of the water.

"In smaller vessels the engines are kept below the water, and are covered by a bomb-proof deck instead of side armour.

"Marine engines working vertically—as it is best they should work—require so much height that only vessels of considerable draught of water can keep the machinery below water, even when there are twin screws. By using three or four screws, greater security might be obtained in vessels of moderate draught.

"Experiments have been made in France, at the instance of Monsieur de Bussy, to ascertain whether multiple screws would so interfere with each other as to give bad results in steaming. Hitherto the experiments have not justified any beyond two screws. . . . . We may therefore expect to see three or more screws introduced into the large French ships."

When the attempt is made to substitute three or four screws for a single one, or for twin screws, the question then is—not to find out if the efficiency is greater from the point of view of speed in proportion to power, but simply to examine if efficiency be not too much sacrificed. The object of increasing the number of propellers is to obtain certain fighting qualities, but not to realise an increased efficiency.

It is not sufficient, moreover, to make sure that the use of several screws is possible, the best relative positions when three or four propellers are employed must also be studied, in order to do away, as far as possible, with the action of mutual interference.

* Introduction by Sir N. Barnaby to a series of Three Lectures entitled "Marine Propellers," by Mr. S. W. Barnaby,—page IX.

Last October, that is to say about the time when the foregoing was written by Sir N. Barnaby, the Council of Construction of the French Marine was considering the advisability of fitting small boats, intended for river service, with four screws. The choice lay between two arrangements, one of which was better adapted to the shape of the stern. It was in consequence of the experiments, which I am now about to recapitulate, that the question arose, not what was the solution most favourable for speed, but, whether the arrangement which was best adapted to the type of boat was not very inferior to the other in respect of efficiency.

When reading this Paper, consequently, the fact must not be lost sight of, that the special object of the experiments in question was to find out whether the advantages promised by the adoption of three screws were not counterbalanced by a serious loss of speed.

The experiments appeared to be sufficiently interesting to warrant the construction of a special steam launch, the "Carpe," the hull and engines of which were built at Lorient. (*See* Plate XXV.)

The lines of the vessel were designed by Monsieur L. de Bussy, the Inspecteur Général du Génie Maritime. They represent to a scale of one-tenth the under water parts of an ironclad 105 metres long, and having a displacement of about 9,600 tons English.

The number of screws is three. When a vessel is fitted with a single rudder, as is usually the case, it is advantageous that the number of screws should be uneven, as that permits of the central screw being placed exactly before the rudder. The "Carpe" did not steer so well when the central screw was removed and the side screws increased in diameter, so as to maintain the propelling surface unchanged.

The arrangement of the screws in the different trials is shown on Plate XXIV.

In suggesting the experiments on the "Carpe," Monsieur L. de Bussy's intention was to elucidate the principal questions which would be raised by the use of a system of propulsion with three screws, and particularly to decide between two different positions of the lateral screws, viz., one position in the same transverse plan as the central screw, and the other situated more in advance, the shafts being also brought closer together, so as to avoid all projection of the propellers beyond the lines of the hull.

A Committee was appointed by our Minister of Marine, in order to control and to analyse the experiments.

The trials lasted for a long time, viz., from June, 1884, to November, 1885. It must be observed that at the outset, there were preliminary tentative experiments, which were inevitable with an apparatus that differed so much from anything which had hitherto been in use.

According to the laws of mechanical comparison, the speed of model corresponding to that of the full sized ship involved a very considerable number of revolutions. As you all know, for corresponding speeds of model, the number of revolutions is inversely proportional to the square root of the number expressing the ratio between the two vessels, the speeds themselves being directly proportional to this ratio.

The speed intended for the ironclad was 17 knots, and the number of revolutions 120.

This high number of revolutions, which rendered possible the use of light engines, was permissible, because the screws being three in number would have a smaller diameter than the screws suitable for a vessel with twin propellers. The resulting number of revolutions for the model reached 380 in order to attain the speed of 5·38 knots corresponding to the 17 knots of the ironclad.

In reality the experiments were not limited to the speed of 5·38 knots. The whole of the steam which the boiler could furnish was made use of in order to push the speed beyond 7 knots, so that the revolutions reached the number of 650 to 700, figures which are altogether unknown for Marine engines.

In view of the difficulty of measuring the power developed with such a high speed of revolution, a system of interior and exterior spur gearing was first considered, by means of which a single engine would communicate motion to the three screws while working itself at a moderate rate. But this arrangement had the disadvantage that it did not permit of the desired direction of rotation being given to the screws. It involved the lateral screws being turned in the same, and the central screw in the opposite direction. But it was desired to turn the lateral screws in opposite directions, because the intention was to compare the system of three propellers with that of twin screws, and the idea of causing these latter to rotate in the same direction could not be entertained.

The use of three independent motors enable that to be proved which had been foreseen in respect of the distance apart of the lateral shafts ; that is to say, by mounting some of the screws in advance of the remainder, the ease of rotation was greatly

increased, and consequently, in case of need, the use of the rudder in making evolutions could be easily dispensed with.

The motor then consisted of three distinct engines, each of which had a single cylinder of the diameter of 124 $^{m}/_{m}$. and a stroke of 96 $^{m}/_{m}$.

The methods ordinarily in use for measuring power would in this case be of no avail, not even those which are efficacious in torpedo boats. Mr. E. Huin, the author of the designs of the "Hoche" and of five other large ironclads of the latest type adopted in France, who together with myself represented the local Council of Naval Construction on the Commission,* pointed out a method of representing the speed of piston on paper, which enables diagrams to be taken even at the most rapid rates. I now present one of these diagrams, which was drawn at a speed of 680 revolutions, that is to say in an interval of less than $\frac{1}{22}$nd of a second. We believe that this is the first time that such results have been attained. (*See* Plate XXVI.)

Another cause which contributed to prolong the experiments was, that the trial trips could only be carried out under good conditions when the weather was absolutely calm. The least ripple was the same thing to the "Carpe" as a rough sea to a large vessel and retarded the speed. In discussing the results it was necessary to eliminate all those in which the sea was not almost calm.

To sum up, it was only by degrees, and by separating the difficulties, that we succeeded in measuring the power, in regulating the flow of steam to the three engines, in counting the total number of revolutions effected in each run on the measured distance, in avoiding currents of water, in keeping the pressure sufficiently constant, in eliminating the influence of the hour of the tide, in maintaining the hull clean, in preventing a permanent list to one side. It would take too long to enter into the details of the experiments which required no less than 49 runs, without counting other experiments. I will confine myself to a *résumé* of the most interesting results.

The results are represented by means of curves, the abscissæ of which are the powers exerted per square metre of midship section, and the ordinates, the speed. The more important of these curves will be useful in rendering what follows more easy to be understood.

* During a stay of two months in Sénégal, I was replaced on the Commission by M. Henry, Ingénieur des constructions navales. The other members of the Commission were, Le Major de la Flotte, President (first M. le Commandant Le Fêvre Dubna, then M. le Commandant Armand Fournier, both Post Captains) and M. Olivier, Mécanicien en Chef de la Marine.

The experiments were divided into series, according to the positions of the lateral screws.

The *first series* comprised all those trials in which the extreme screws were in the same transverse plane as the central screw.

In this series the following propellers were tried successively :—

Three screws of 44 centimetres diameter, the ratio of pitch to diameter being 1·25.

Three screws of the same diameter, the ratio of pitch to diameter being 1·55.

Two large screws mounted on the lateral shafts having, like the first, the number 1·25 as ratio of pitch to diameter, but having a diameter of 54 centimetres, so that the sum of the areas of the circles described by the extremities of their blades equalled the three corresponding areas of the smaller screws. For the sake of simplicity we will say that the propulsive surface was the same in each case.

Finally the small lateral screws were put back by themselves, and the first group of experiments tried over again, the two pitches being tested turn about. This last formed a series complimentary to the first.

In this series of tests, the comparison of the experiments made with the small screws rendered it possible to take account of the influence of ratio of pitch to diameter in a system of three screws.

Contrary to what might have been expected, the greatest pitch, although its ratio to diameter was very high, turned out to be the most favourable to speed. This result was due to the fact that the screws mutually influenced each other at high speeds in consequence of their proximity, and this influence which was scarcely perceptible at about 500 revolutions was very marked, with the screws of small pitch, when driven at about 150 revolutions more than the above number. The proof of this was forthcoming when the central screw was suppressed in the experiments complimentary to the first series ; in this case the propellers were more completely independent, and the screw of smaller pitch had the advantage. It was also ascertained that even with this pitch, an increase of speed was gained when the central screw was suppressed. (*See* Plate XXVI.) Further experiments confirmed this result.

On the other hand the best of the two sets of three screws, above mentioned, hardly attained the efficiency of the two large screws. This fact ought to occasion no surprise, for the substitution of the double for the triple system of screws only implied that a

propulsive surface, equal in the two cases, was in the latter instance more completely separated from the hull.

It is proper also to remark that the use of two large screws in an ironclad involves an increase of the draught of water, for the circumference described by the edge of the blades descended lower than the keel. (*See* Plate XXIV.)

After this first series of experiments, some trials were made of three screws so arranged that the lateral propellers were mounted in advance of their supports, and 33 centimetres in front of the position assigned to the central one. We have designated this the *intermediary series* of experiments.

The propellers being then at a greater distance from one another, the effect of an exaggerated speed of rotation made itself less felt, as indeed might have been expected, and the advantage remained with the screws of small pitch. (*See* Plate XXVI.)

It may then be concluded that, whenever the independence of the propellers is sufficiently provided for, the pitch, whose ratio to diameter is 1·25, should be preferred to a more rapid one, as well for systems of three as of two screws.

Finally, in the *second series* of experiments, the lateral shafts were brought nearer together by 65 $^{m}/_{m}$, while still remaining 33 centimetres in advance of the central screw, so that they were completely covered by the exterior contour of the hull. The supports of the shafts were then placed in front of the screws in the usual way. The results differ but little from those obtained in the *intermediate series.* (*See* Plate XXVI.) However, in analysing the results, it is to be remarked that at low speeds the latter position is less advantageous, which tends to prove that, in this case, it is the injurious effect of the proximity of the hull which affects the result. At high speeds on the contrary the latter arrangement proved to be superior, which result must be attributed to the better relative position of the supports. It is easy to understand that this influence predominates at high speeds, since the resistances to progression increase at a much more rapid rate than the speed.

If we reflect that rudders have much less influence when placed before the screw than when behind it, it will be understood that a body in the neighbourhood of the screw is especially prejudicial when it acts as an obstacle to the water which the latter projects.

Reverting to an experiment, which was, I think, originated at Brest about fifteen years ago, by a Naval Architect, I mounted in the open air a small screw of 6 centimetres diameter, the shaft of which was driven by means of a pulley from the workshop shafting. When the speed of rotation was sufficiently rapid—it was 540 revolutions in the experiment at Lorient—the flame of a candle was successively brought near to all the points in the neighbourhood, and the direction taken by the flame noted. The following facts were proved.—The screw draws air from all points situated in front of a transverse plane passing through the extremity of its blades, and even a little from behind this plane; it then projects this air in a truncated cone, or more correctly a sort of truncated pear, the small base of which is a circle equal to the diameter of the screw, the generating line being inclined at the outside at an angle of 30 degrees at the start. Properly speaking, the surface generated by this line is a zone of unstable equilibrium, within which the flame is repelled, and on the outside of which the tendency to attraction is made manifest.

It is important to remark that in this case the screw turns in a fixed position, and if we had to do with a vessel in motion the truncated cone would be drawn out, so as to approach the shape of a cylinder. I had the opportunity of verifying this induction in a voyage in the tropics, during nights when the sea was phosphorescent. The mass of water thrown back by the screw appeared to be enclosed in a cylinder of the same diameter as the propeller, and had the appearance of a sort of a luminous spiral faggot, which I could not better describe than by comparing it to a cable of very slow pitch which continually untwisted itself in proportion as its length increased.

From this observation it results that a bracket placed in front of the screw allows the fluid to reach the latter from a multitude of points, while if placed behind it intercepts a slice of a truncated cone proportional to its own section.

The facts also show why there is an object in putting the screws solely behind the vessel, and not simultaneously in front and behind, as has sometimes been attempted latterly in England.

In short one may conclude from this experiment that if the three screws interfere with each other at great speeds, when they are placed in the same transverse plane, it is not because they send water to each other in consequence of the phenomenon designated by the name *dispersion*, but rather because they dispute with each other for the possession of the water which they draw upon.

Thanks to the new supports for the lateral shafts, the screws of small pitch which, as we have seen, showed themselves to be most advantageous when the outside propellers were carried forward, attained high speeds and surpass even the best of the screws tested in the first series of experiments. (*See* Plate XXVI.)

The second series was completed by the test of the two large screws already experimented upon, which were mounted in the new positions of the side screws. The comparison was not favourable to them; their performance was even inferior to that of the three screws of rapid pitch. (*See* Plate XXVI.) The order of merit in the first series was completely reversed in the second, but the difference between the effect of the slow and the rapid pitch for the three screws, though changed in direction is much less marked than in the first trials.

It is proper to observe that even in this case the large screws reached down lower and projected higher than would be suitable in the case of a large ship.

The results of the experiments may be summed up as follows:—

With vessels of the form of the "Carpe" three screws are, from the point of view of speed, very nearly equivalent to two screws of the same propulsive surface, and immersed to the same depth, when the most favourable position is chosen for each system.

Twin screws, when under the above mentioned conditions of surface and immersion, occupy in all cases positions which are disadvantageous in their relation to the requirements of war, and which are difficult of access in roadsteads and ports; on the other hand, it is possible to find for three screws, positions which are without inconvenience from the points of view both of fighting and navigation.

In short, a circumstance which is most interesting in the case of three propellers is, that of all the positions which have been tested, those which are best sheltered by the hull are also those most favourable to speed.

In a word, for a vessel of similar form to the "Carpe," the best plan appears to be to employ a system of three screws placed like those in the second series of experiments, and having a ratio of pitch to diameter differing but little from that ordinarily in use for vessels having single or twin screws.

DISCUSSION.

Mr. J. I. Thornycroft: My Lord and Gentlemen, I have very much pleasure in making a few remarks on this Paper, because it treats of a subject that is of great interest to me, and I feel it is of great interest to naval architects generally. As ships have increased in size, the necessity of diminishing the draft of the propeller, in contra-distinction to the draft of the ship, has become more and more apparent, and the necessity of keeping the propeller blades within the space protected by the vessel's hull, of course, in merchant ships, has become very important in general navigation; now that we shall have, besides the Suez canal, another canal cutting America into two—this consideration becomes still more important. One thing that strikes me in this Paper is the consideration of the mutual interference of one propeller with the others. And it seems to throw some light on some experiments that I have made. Some years ago I designed a vessel which I am afraid I have spoken of too often, but in this vessel--it was a twin screw—the propellers were housed up into the hull to keep them immersed, although partly above water mark, and I found that the efficiency of this boat was greater than might have been anticipated, and arguing from what had taken place in that, I was induced lately to recommend the adoption of the plan in building some torpedo boats for the Admiralty, which Figs. 1 and 2 illustrate. Those figures represent the stern of the torpedo boat

Fig. 1.

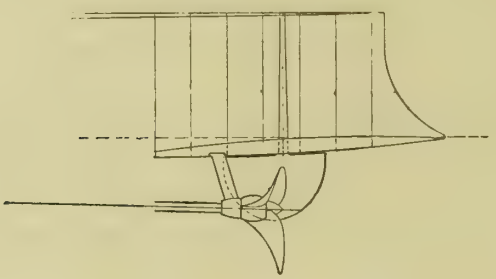
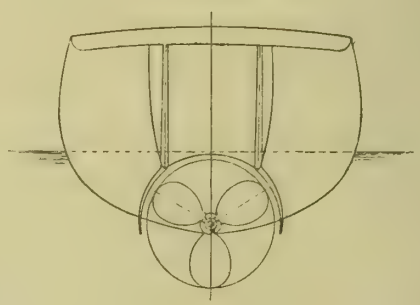


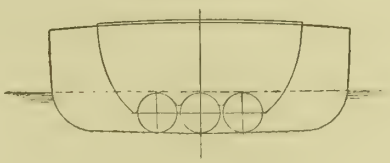
Fig. 2.



and the mid-ship section. There is a circular line on Fig. 2 which represents the circumference of the circle enclosing the propeller. In those boats it was wished to keep the draught of the propeller as small as possible, and to facilitate steering it was wished to carry a rudder immediately on either side of the propeller, so situated that whether the boat went ahead or astern, the stream from the propeller would strike one of the rudders. What I had anticipated in that trial was, that we should certainly get very superior steering qualities, and I did not think that the efficiency would be marred to an extent to counter-balance the gain in steering. But a very curious result has come out of that, which is, that instead of the efficiency of the propulsion being diminished, so far as we can find out, the efficiency of the propulsion is actually increased. In these torpedo boats, in order to provide for variation of draught above the proper load line, the stern has been modified in an unusual way; the greatest length of the boat is at the load line, and for increased immersion the water is allowed to

flow smoothly over the arched section so as to avoid any surface making a very large angle with the line of the vessel's motion. If it were attempted to lead up the channel from the propeller to any considerable extent, I should anticipate great loss of pressure on this part of the channel, and consequently resistance to the vessel's proper motion. There is one remark in the Paper about the number of revolutions of the engines being quite unusual in marine engineering. In the second-class torpedo boats we have driven engines over 600 revolutions, many of those engines have been now working for a considerable time at that speed, and although these engines indicate over 150-horse power, they do not seem to give any trouble. The fact is if you run them short of oil, they get hot and they speak for themselves; but if you drive them properly, they keep in perfect order and do not seem to give any trouble. There is one passage in this Paper which is important, and that is as to the prejudicial effect when any object is put behind the screw, in contradistinction to being put before it I think that might have been anticipated. It is quite evident that as the speed of the water is considerably increased in passing the propeller, that any obstacle behind the screw will encounter much more resistance than it would do forward of it. To illustrate the action of a propeller, he speaks of how a flame of a candle was deflected when a propeller was run in the air. Being in their own element, they were better able to observe what was going on. But in this case, as he remarks, it was in still air, and the direction of the currents produced at different points is much influenced by the fact that it was still air; and we have to consider in a propeller, not what will take place in still air, but what will take place when we only accelerate the speed to a limited amount. No doubt, judging from the luminous water caused by disturbing the animals in it, he was able to observe in the sea that the propelling stream was drawn out in a form resembling a cylinder. I am not sure, however, whether that would give a perfect indication of what was going on, because, having observed phosphorescent water myself a good deal, I have noticed it is only when the little jelly fish are first disturbed they give a flash and then they get tired, and you cannot expect them to keep on giving a light as often as they are disturbed. I wish to call the attention of the meeting to another diagram here, and that is the diagram showing a section of a proposed model launch we intend building, to test the effect of three guide-blade propellers; you will see from the section, Fig. 3, that they are not only covered by

Fig. 3.



the hull, but very much covered if you consider the width. The point I wish particularly to call attention to in that design is this, that although in the ordinary screw propellers where the stream is accelerated before arriving at the propeller, one propeller will rob the other of water, it is not so in this case. If a guide-blade propeller is properly designed so as not to affect the motion of the water until it is within the apparatus, the one cannot rob the other of water for propulsion, and I do not anticipate the trouble experienced from this cause. I think for shallow vessels it will be found that

that particular propeller, perhaps using as many as three, is very suitable. I have to thank you, Gentlemen, for so kindly listening to me.

Mr. J. McGregor : I am sorry, my Lord and Gentlemen, to come here to some extent unprepared with full particulars, as I might have had the results of experiments with triple-screw propulsion on a considerable scale. We had three steamers constructed last year with triple screws. They have given very extraordinary results, the main features of which I will put before you, but I have not got complete details. I think, perhaps, I ought to indicate, before I go further, the appearance of the ship as per Figs. 1 and 2.

FIG: 1.

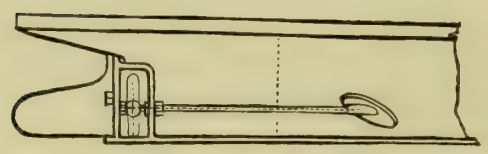
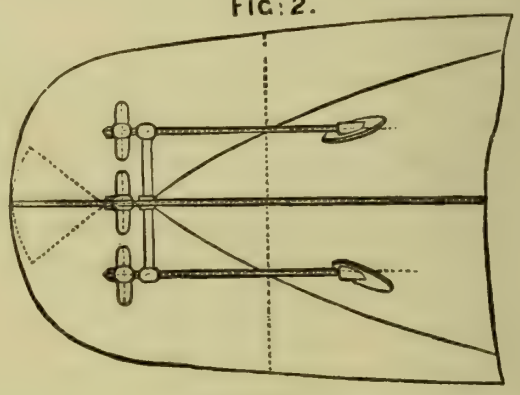


FIG: 2.



The vessels had unusual conditions to fulfil. They required to have a comparatively shallow draft, and to go about twelve miles per hour in very tortuous rivers ; altogether the conditions gave us very little latitude, and as stern-wheel steamers were not considered suitable at the time, we adopted, in order to get the requisite resistance to the power required, three screws. The vessels were 98 feet long and 18 feet beam, and the screws were each of them 2 feet 9 inches in diameter. When they were tried they went at ten miles an hour, but when we stopped the central screw no appreciable effect was produced in the speed, which is the same result as that experienced by M. Marchal, who says : "It was also ascertained that even with this pitch an increase of speed was gained when the central screw was suppressed." There was no difference in the speed of the vessels when the centre engine was stopped, *i.e.*, they went at the same speed with two as with three engines working. But as we wanted somewhat more, we have sent to the vessels abroad an arrangement for increasing the efficiency of the screws with this object in view. Unfortunately, they are working at some distance from where they can easily be experimented with and attended to, and as they are doing the work they have to do satisfactorily there does not appear to be the inducement to lay them up for alterations there otherwise would. The alteration alluded to above consists of covering each of the screws with a shield and drum attached to the ship. My object being to prevent the action of one of the side screws affecting the centre one, or the centre one affecting the sides ones ; as, although the screws were fully immersed at the trial when they were all working, they so acted one on the other as to considerably diminish their efficiency. We have sent out an alternative arrangement of extending

the central screw shaft, and placing the central screw behind the rudder, and perhaps on some future occasion I may be able to give you results of trials with these arrangements. We have tried screws of different pitches and diameters, but there is not much difference from altering the pitch; altering the position of the side screws forward also has little effect. Unfortunately, the vessels are in a very out of the way part of the world, and it is difficult thus to get experiments satisfactorily carried out. This is a case of triple-screw propulsion, and as there are very few, if any, such examples, I have brought it to your notice. We expected nearly the same effect from each of the screws that would have been obtained from one individually, and I believe if we can isolate the action of each, and prevent its affecting the other, we shall have very nearly that effect. As it stands, the efficiency of the vessel with two screws is remarkable.

Sir Nathaniel Barnaby: I may perhaps be allowed to say, my Lord, that the words which were quoted in M. Marchal's valuable Paper, from something which I wrote, did not refer to such screws as those which were used in the French experiments. The propellers which were in my mind, as the context will show, were the propellers which have been experimented upon and used successfully by Mr. Thornycroft, in which there are two things which are very important to success. One is that the water, meeting the screw, is not accelerated at the time it first meets the screw at all. The acceleration takes place entirely after it is within the apparatus. I think these French experiments show it is an essential condition of success with three or more propellers. The other thing which Mr. Thornycroft, has already referred to is, that the propeller to which I invited attention is absolutely inclosed in a cylinder. The experiments, so far as they go—that is to say, in France—do not really throw much light, if any, upon the question which I raised in the Paper which has been quoted from. The real point has to be settled by the experiment which you see there on that diagram on the wall. That is an actual trial which is to be made with three screw turbines. If that is successful, it will be successful because the propeller is one which is suited, as I think, admirably for the purpose to which it is there applied. There is no acceleration of the water to begin with. Acceleration begins when the water is within the apparatus; and, secondly, the interference of the screws with each other's action is absolutely prevented. My own belief is, that we shall see in the future the means given to us of largely increasing the number of propellers without interference with each other's action; that we shall be able to use engines very much faster and smaller; that we shall be able to keep the machinery below the water, and that there is in that propeller a very great future to which the members of this Institution would do well to turn their attention. I do not think that we should, any of us, have been very much surprised at what has happened in the case given us by Mr. McGregor. What I rather mean is, that those who have thought much about it would not have been much surprised. Mr. White, Mr. Froude, Mr. Wright, and I have talked over, years ago, the question of using three propellers instead of two in large ships. Mr. White and Mr. Froude, I know, were of opinion that that would happen which is shown to have happened; that it required some change in the propeller itself, and it is that change, which lies before us, in order to get any success with the multiple propulsion. That we shall have success with multiple propellers I think is absolutely certain.

Mr. W. H. White: My Lord, Sir Nathaniel Barnaby has referred to the consideration we gave this matter some years ago at the Admiralty. Of course there are very obvious advantages in using

very quick-running engines and in having them well sheltered in a war ship, and incidentally there is this advantage, that if there are a good many engines in a war ship cruising at a very moderate speed, you can throw out of gear some of the engines and get rid of the waste work. There are several ways of carrying out that principle, but if the principle is associated with multiple screws and very small quick-running engines, it no doubt gets its best exemplification. But there are disadvantages in such an arrangement, and the conclusion we reached at the Admiralty some years ago was two-fold. First (what Sir Nathaniel Barnaby has explained), that with multiple screws special propellers should be associated, and that the most promising form of propeller for such use was Mr. Thornycroft's guide-blade propeller; I said so publicly, in this hall, four or five years ago. That carries with it a special construction of the stern of the vessel, in order that the system may be utilised. The second conclusion was, that for the speeds and the powers and the available draughts which we were working with at that time, it was wiser on the whole to keep to the twin screws. That conclusion may not always hold good. I quite share the opinion of Sir Nathaniel Barnaby that if we go up in speeds, within a certain working limit of draught, we are likely to be driven to a variation in the form and arrangements of our propellers, and I think that may prove true not merely of war ships, but of ocean-going steamers. In a discussion which took place on Mr. Thornycroft's excellent Paper on torpedo boats at the Civil Engineers—I do not know how many years ago—I ventured to say so, and he did not contradict me. If the experiments, which I am happy to hear are now projected, succeed, as I sincerely trust they may, we shall hear more of that matter, and we shall see another opening for progress in the constant endeavour to gain higher, and still higher speed in steam navigation. I wish to speak now more particularly with regard to the experiments. M. Marchal's experiments were made within a limited range, and intentionally made so. On page 232 he explains that (in the passage following the quotation from Sir Nathaniel Barnaby's preface) it was not that they expected to get any economy of propulsion, but they wished to get greater manœuvering power, and they were prepared if necessary to sacrifice a little efficiency in order to get greater handiness. Now, of course, there have been experiences, not of a very happy nature, as regards the steerage of some twin-screw ships, French apparently as well as English; but I think, speaking broadly, we may say that in the well-formed twin-screw ship the steering has been satisfactory. I think that is not going beyond the truth. If in twin-screw ships the steerage has not been so good, there has generally been something in the form or the special arrangement to account for it. The limited conditions of the designs have introduced bluntness of ends, or peculiarities of form which have not been conducive to good steering. There is, no doubt, and I think the French designers are perfectly right in thinking that if there is a central rudder with three screws, then under many circumstances there would be greater manœuvering power than with two screws set in the ordinary way, especially when getting up speed or reversing. I am glad to see some naval officers here who may be able to speak on that point, but the general result of these experiments does not seem to me to be such as in any way to cast a doubt on the conclusion we arrived at in the Admiralty some time ago—namely, that within our present limits of draught, speeds and powers, we could do as well with twin screws, and on the whole somewhat better, than could be done with three screws. The experiments described here are extremely interesting in this respect, that they are really model experiments in propulsion. I am sorry I do not see Mr. Pearce, of John Elder and Company, present, who in connection with the construction of that famous

vessel the "Livadia," actually did exactly what the French have more recently done, and constructed a model "Livadia" which resembled the "Carpe" in this respect, that she also had three screws. Mr. Pearce made progressive trials with that little vessel as the basis of the design, and the complement of the model experiments made in Holland to determine the fluid resistance to the form. I remember going afloat in that model "Livadia," and studying her features with very great interest, and in the "Livadia" herself, so far as I have ever heard, with the limited draught she had, taking the model experiments on resistance as a basis, they had no reason to be dissatisfied with three screws. But in the "Livadia" there was a very special width of ship, as is well known to gentlemen present, and a very considerable distance athwartships between the outlying screws and the central screw, and interference was probably reduced to its minimum in that case. This Paper describes experiments which are in some respects inconclusive. There is no scale of comparison that I know of, and I hope we shall hear more of that from Mr. Froude, which would enable us absolutely to trust the results obtained with these small screws when passed on to the larger screws of the full-sized vessel. I think that would in some respects vitiate the comparison. Then again, in making variations in longitudinal positions in the screws in the first series, as they had the struts fixed on a certain athwartship line, all the experimentalists could do was to move the screws forward on that line, that is to say, to put them before the strut. There were no experiments made with the far distant screws (measuring from the middle line plane of the boat) and with the strut in its proper position in relation to the screw propellers. I am of opinion that if in the "Carpe" the experiments had been made with these outlying screws and the struts in the proper position, with a greater athwartship distance between the propellers, still better results might have been obtained. Of course they did what was possible within their limits. But I think a most instructive series of experiments is what is called the second, where the distance from the hull of the ship of what would correspond with the twin screws of the "Carpe" was very sensibly decreased. I remember, some years ago now, Mr. Froude giving us very valuable information as to the considerable limits which might be adopted in the longitudinal position of twin screws in relation to the hulls of ships, that is to say, the length of the outboard shafting without much sensible effect on the propulsion. I think the results obtained here in the second series of experiments really show that the improvement in the propulsion was no doubt effected not merely by the change in the athwartship position of the outlying propellers, in relation to the middle line propeller, but also by a better utilization of the "frictional work" of the boat. I should like, my lord, to add, that the Institution owes a great debt to M. Marchal for giving us experiments of this precise and valuable nature, and also to M de Bussey, the head of the constructive corps in France, whose permission of course was essential to this production. It is a great compliment to us that information of this nature, bearing on the latest designs of the French national fleet, should be put before us in this complete and exhaustive form.

Mr L. Benjamin: My Lord, if I venture to make a few remarks on this subject, it is because I, in connection with Mr. Taylor, have during the last year made a great many investigations which bear on the point under discussion. These investigations are not yet in such a state as to enable us to publish them; however, I shall be able to give you one result of them. In the Paper we have to read

later on—though I will not anticipate anything in connection with that matter—we give an example of a method which we propose for arranging the screws of shallow-draught steamers, in a manner which, though similar to what Mr. Thornycroft described just now, as well as to what M. Marchal described, is still on a principle different from both. In our lifeboat, of which the model is before you, you will notice that the screws are situated at about half the vessel's length in parallel-sided grooves which extend all the way fore and aft, and in designing this the difficulty arose to form the grooves so as to create no hindrance to speed. This same difficulty must in a smaller degree occur in the afterbody of a boat with a body plan such as M. Marchal shows in his drawings. If the longitudinal curve of sectional areas of a vessel like that of M. Marchal is constructed, I presume it will offer a fair curve. But the ordinates of this curve will in a case like that be composed of several elements corresponding to the elements of which the hull is composed—I refer to the different parts into which the hull is divided for the formation of the bosses. If these elements of the ordinates are plotted down as separate curves, it is most likely—as far as I can judge from looking at the body plan—that they are not fair in themselves, although their summing up will result in a fair curve. Now this is what I was going to point out. The longitudinal curve of sectional areas, and the curves of the components of its ordinates, are in my opinion the most important criterion of the speed qualities of a vessel. It is impossible that a vessel which has the slightest unfairness or discontinuity in any of these curves can be as advantageous for speed as one where they are only composed of fair and continuous elements. To explain clearly what I mean I have again to refer to the model of the lifeboat which you see before you. The transverse section of the boat is composed of two outside parts of common shipshape, two parts containing the grooves, and a middle piece. The highest points of the grooves are in the way of the propellers, they drop in fair curves amidships, and again towards the ends. If I now draw the longitudinal curve of sectional areas of each of these parts, and the curve of sectional areas of the total, each of these ought to be fair and continuous. The most difficult point occurs where the grooves end, as without the greatest care being taken there will be a sudden change in the curves at this point, which must indicate a hindrance of speed. This will occur in a smaller degree in Mons. Marchal's section, and also in what Mr. Thornycroft sketched on the board just now. It is however possible to shape the different parts so as to avoid any such sudden change or discontinuity, and even in a boat similar in shape our model (although on the first glance it appears to be not well adapted for speed) I believe that if these curves are properly shaped there can be nothing which is detrimental to speed. Of course, that question cannot be dealt with quite independently of the position and nature of the propellers, but whatever theory may be adopted for the relation existing between these and the ship's hull, it cannot interfere with what I have pointed out as to the value of the curve of sectional areas and its components as a criterion for speed. The waterlines are inferior for this purpose because the water does not flow along the same, but the curve of sectional areas indicates how the water is displaced from section to section while the vessel moves forward.

Captain J. D. Curtis, R.N.: My Lord and Gentlemen, I will not keep you many minutes, I do not profess to be a scientific man, but I am a man of some little observation. It is said in this paper that

screws will probably do away with rudders: I hope not, unless they are fitted with lobster tails. I think, on the contrary, that each screw should have a rudder close behind it. That is where we want the efficiency in manœuvering, or else let us have quarter-rudders as the ancients used to. The efficiency of the screw depends on its position in relation to the after part of the ship; that is, the ship must take a certain amount of water with her, which sailors call dead water and other people call following water, if the screw is so placed; without that influence it is independent of the ship's water. The ship must have its water like the train has its air to follow it. With respect to the central screw, if you have three furnaces the two fires of the outside furnaces nearest the doors will burn much brighter than the central one because they take the air; in like manner the two outside screws can work better with the water than the central one, having a better supply. I have heard nothing of the "Popoffka." I heard last Monday that she went just as well with two screws as she did with the eight. Mr. Thornycroft gets his increased speed by giving greater resistance to the screw, confining the water within his guide-blades, and the stern which overhangs the screw, and it does not create that surface wave. Now, a gentleman here has put a shield over this. No doubt many gentlemen here were perfectly familiar with the late Robert Griffith—he introduced a peculiar double shield, but, could not get the public to take it up, although it is good. I have heard of these boats shaking their sterns almost off, like a duck coming up from diving. Here is this shield which will give you resistance and increase the speed with less force. We tried it above Teddington Lock, and you might have put a tumbler of water down there and hardly have seen a bubble on it. I am not telling you what I think, but it is a fact. It is difficult to get the public to believe in facts, but you cannot get over facts. Mr. Thompson, who is unfortunately dead, introduced that tunnel for the screw to work in his lifeboat. There is one of them in the United Service Institution, and also one can be seen at South Kensington. I am rather hasty you see, but I feel what I say. If you go to South Kensington you will see the shield and protector. The protector was tried at Swansea and was a perfect success. The shield is only an eighth part of the protector, but they will not adopt it until they find their screws jammed in action. Of course they have to keep down their expenses, and I think the clerks are our enemies, not our officers or my lords.

Rear-Admiral the Hon. E. R. Fremantle: My Lord, the few words I have to say I think scarcely render it necessary for me to come to the tribune, but naval officers and others who have had experience of ships that have been fitted with two screws, naturally should give the result of their experience on this subject; and, no doubt, as the two screws have not been adopted so generally in the merchant service as in the navy, it is fitting that a naval officer should speak on this question. Now the advantages and disadvantages of two screws over one screw are somewhat in point, and for a moment or two I will just touch on the subject. There are evident and obvious advantages in many respects of the two screws over the one, and one of the principal advantages from a naval point of view is that your ship is divided in the centre—longitudinally, and that the two screws are in separate engine rooms, and that it allows in fact of a dual system altogether. There are some disadvantages, and the principal disadvantage, as far as I am aware, is one which has been alluded to—namely, the projection of the screws beyond the ship. Any officer who has commanded a double-screw ship and has had to

go alongside wharves for the purpose of coaling or for any other purpose, must be aware that on leaving these wharves or piers, it is very necessary to be careful not to move your engines until you are well clear, because otherwise, as happened to me on one occasion when I first commanded a double-screw ship, you are liable to do a slight injury to one of the fans. I only mention that, because that bears distinctly on the question of having three screws, and consequently, as was mentioned in the Paper, there are distinct advantages as long as they can keep the screws clear of the ship, and as was mentioned by Sir Nathaniel Barnaby in the preface of this Paper, the engines can be smaller, and altogether we can keep things below the water line. Now the question of steering is, therefore, the only question on which there can be much difference of opinion as regards the advantages of the two screws over the one. I leave out of consideration the question which did bear upon it very considerably some years ago, namely that in a single-screw ship you could enable the screw to be lifted, but with a double-screw ship you could not lift the screws. That has all gone, of course. We are dealing now with a ship propelled solely by her screws. Touching on the question of steering, certainly, that is a question on which I should like to say a word or two. With the double-screw ships, as far as my experience goes, I am bound to say there was a slight tendency somewhat to sheer. With the single-screw ships there was not that tendency, and you could more depend on a single-screw ship making her circle, under certain circumstances, as you expected, than you could on a double-screw ship. But I am bound to say that the principal reason for that was in my opinion that the single-screw ships which I have commanded have had keels, and the double screw-ships have not had keels. But there is also another reason. Unquestionably, your engines in a double-screw ship may not be always running at the same speed exactly. Of course, the chief engineers will invariably tell you, if a question is asked, that they are running at the same speed. There are circumstances, not quite under the control, very often, of the best of chief engineers, and occasionally there may be a little more steam in one set of engines than the other, and under those circumstances it is very natural that, if for instance your port engine is running faster than your starboard engine, and you put your helm a-starboard, you find the ship does not make exactly the curve you expected. I think all those subjects are of comparatively small importance when the fact remains that by using the screws themselves you can make the ship steer, practically, almost exactly as you like. As regards the practical question, I can say honestly, that when I had the honour of commanding one of the largest vessels in her Majesty's service, a twin-screw ship, I felt perfectly safe in undertaking any operation and bring her into any harbour, however narrow, because she was certainly a ship that steered remarkably well, and therefore I entirely agree with Mr. White that, speaking generally, any difficulties which have been found in steering the double-screw ships, have been due decidedly to the form of the ship. That is a question on which, undoubtedly, the naval architect is far more competent to speak than I am. I would sum up therefore by saying that, personally, I am perfectly satisfied with the double-screw ship; at the same time, I do think that if we had three engines and the rudder just abaft one of the screws, the ship would be a trifle more steady in her steering. But then a question arises as to several disadvantages which must exist in multiplying the number of screws. There is a distinct advantage, as I pointed out, in dividing your power, and there is a distinct advantage in having two engine rooms, but of course we must have some communication between those two engine rooms, and when we come to three engine rooms—I call them three engine

rooms, because you must assume that the naval architect would divide his engine-rooms by longitudinal compartments one from another—there may be some slight practical difficulty. When you come to a greater number than three, and come to multiple screws and consequently multiple engine-rooms, which you would have in a large ship, there would be an increase in difficulty in keeping up a constant communication between all these engine-rooms, if I may speak of them in the plural, and keeping them properly under command of the officer who would be in charge of the engines. On the question of compartments I should like to have heard just a few words from those who are so very competent to give an opinion, namely, the naval architects.

The PRESIDENT: Then, Gentlemen, I take it that you wish that this discussion should now terminate. I am quite sure that I shall have your sanction in authorizing our Secretary to convey your best thanks to M. Marchal for his Paper, which has created a very useful and interesting discussion, and also to M. de Bussy for the permission which he has given to M. Marchal to convey the information which it contains. I am sure we must all feel that our connection with scientific men abroad is of very great value to our discussions—that we owe them our thanks, candidly and openly, for giving us the opportunity of discussing the latest production of their intellects. We have the greatest possible respect for the talent of foreign engineers, and I think we ought to convey formally our thanks (and I only ask your sanction to authorize the Secretary to convey those thanks) both to M. Marchal, and to our old friend M. de Bussy.

THE DETERMINATION OF THE MOST SUITABLE DIMENSIONS FOR SCREW PROPELLERS.

By R. E. Froude, Esq., Associate Member of Council.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

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§ 1. At the meeting of this Institution in 1883, I read a Paper entitled, "On a Method of Investigation of Screw Propeller Efficiency." In that Paper, the nett-efficiency of a screw propelling a ship was treated as the product of two component factors, of which the one expressed the efficiency that the ship's screw would possess if working under analogous conditions in still water; the other, the modification of this efficiency due to the conjunction of the screw with the ship's hull. The first-mentioned element was termed, for distinction, the "screw-efficiency" proper; the second, the "hull-efficiency." My previous Paper dealt mainly with the "hull-efficiency" element; the present Paper will deal mainly with the "screw-efficiency" element, since it is chiefly this latter element which is affected by variations of size or proportions of propeller.

§ 2. So far as I am aware, this treatment of the general efficiency as divided into these two elements is not one which is as yet generally familiar to naval architects, and I think it may very possibly appear at first sight to be an unnecessary complication of the problem; moreover, it must be admitted that in order to pursue this treatment we have in some slight degree to strain the facts into accordance with certain assumptions. I therefore think it desirable to explain at the outset that my chief reason for maintaining and insisting on this treatment is simply this, that it is only by such a sub-division of the problem that an experimental investigation capable of yielding instructive results for a sufficient variety of hulls, combined with a sufficient variety of propellers, may be brought within reasonable limits.

§ 3. The treatment referred to is fully explained in my 1883 Paper; but since a clear understanding of it is essential to the proper understanding of the present Paper, and since

it may be illustrated for present purposes perhaps more intelligibly than in the previous Paper, I will here go over some of the ground again.

§ 4. Let us then begin by considering the case of a ship being propelled by a screw propeller at a speed = V. The ship's resistance proper to this speed, apart from any effect of the propeller, (*i.e.* the "nett-resistance" as it has been termed), is = ρ. The screw is working with revolutions per minute = R, and delivering a thrust = T (greater than ρ by the amount of the "augmentation" of resistance). The power consumed by the screw in maintaining this thrust, is less than the measured I.H.P. of the engines, by the amount absorbed in friction of engines and shaft, &c., an amount which, at full speed, may be taken as a constant proportion of the I.H.P.; and since we are not here concerned with absolute efficiency so much as with the differences in efficiency caused by differences in propeller conditions, I will venture with this apology to simplify matters here by neglecting the engine friction, so as to be able to apply the familiar expression I.H.P. to the power actually delivered to the screw.

§ 5. The general, or nett-total, propulsive efficiency, is expressed by the power realised (viz., the product of "nett-resistance" and speed, = ρV, or E.H.P. as it has been termed), divided by the power consumed (or I.H.P.). This nett-total efficiency is expressed by the familiar "propulsive co-efficient" $\frac{\text{E.H.P.}}{\text{I.H.P.}}$

§ 6. Now suppose the ship to be removed, or rather to become, so to speak, a phantom-ship, capable of resisting the thrust of the screw, but creating no disturbance in the water; the speed to remain the same, *i.e.*, = V; and the screw to continue revolving at the same revolutions per minute R.

In this case we know that, for want of the following current left by the real ship, the thrust exerted by the screw of the phantom ship, working with revolutions = R, will be much less than = T. The thrust could be made to = T by increasing the revolutions, but neither so could the conditions of operation of the screw be considered to correspond to the conditions which obtained in propelling the real ship. On the other hand, the thrust may be increased without altering the revolutions, by reducing the speed, and thus at some lower speed which we will term V_1, the screw propelling the phantom ship may be made to exert the thrust T with the revolutions per minute R.

§ 7. The screw, then, propelling the phantom ship, and working in undisturbed water

at the speed V_1, corresponds in condition with the same screw propelling the real ship at the speed V,—at any rate in the two very important respects of having the same thrust and revolutions per minute. If the following current left by the real ship were of uniform forward speed throughout the area operated on by the screw, the correspondence would clearly be absolute, and there could be no doubt that the I.H.P. driving the screw and maintaining the thrust T in undisturbed water at the speed V_1, would be exactly the same as that driving the screw and maintaining the same thrust T behind the real ship at speed V. That the following current is not uniform we know; but this defect of uniformity is only important in respect of efficiency in so far as it may affect the I.H.P. with which the thrust T is maintained, and it is a fact, proved by multitudinous experiments on screws working behind models and in undisturbed water, that the I.H.P. of a screw propelling a model, differs extremely little from that of the same screw exerting the same thrust at the same revolutions per minute in undisturbed water.

§ 8. I therefore contend that in any case such as we have supposed, we may treat the I.H.P. of the screw propelling the phantom ship with thrust = T and revolutions per minute = R, at speed = V_1, as identical with the I.H.P. of the same screw propelling the real ship with the same thrust = T and revolutions per minute = R, but at speed = V.

§ 9. Now, what is the efficiency of the screw propelling the phantom ship? The speed is V_1, and the resistance is equal to the thrust T, consequently the E.H.P. of the phantom ship is TV_1. This, to distinguish it from the E.H.P. of the real ship, is termed "T.H.P." or "Thrust Horse Power." The efficiency, then, of the screw in undisturbed water, working under conditions corresponding to those of the same screw propelling the real ship, is $\frac{\text{T.H.P.}}{\text{I.H.P.}}$. The efficiency of the same screw propelling the real ship is $\frac{\text{E.H.P.}}{\text{I.H.P.}}$. The "hull-efficiency," then, or, in other words, the ratio of the efficiency in propelling the real ship to that in undisturbed water, is

$$\frac{\text{E.H.P.}}{\text{I.H.P.}} \div \frac{\text{T.H.P.}}{\text{I.H.P.}} = \frac{\text{E.H.P.}}{\text{T.H.P.}}.$$

§ 10. E.H.P, we have seen above (see § 5), to be $= \rho V$; T.H.P. (see § 9) = T.V.; $\therefore \frac{\text{E.H.P.}}{\text{T.H.P.}} = \frac{\rho V}{TV_1}$ This, again, may be written as $\frac{\rho}{T} \times \frac{V}{V_1}$. Thus the hull-efficiency naturally divides itself into two factors, of which the one (*i.e.*, $\frac{\rho}{T}$), represents the amount by which resistance is less than thrust, is therefore termed the "Thrust-Deduction" factor, and implies loss in efficiency. The other (*i.e.*, $\frac{V}{V_1}$) represents the excess of speed of ship over speed

of screw through the water it works in, is therefore termed the "wake" factor, and implies gain in efficiency.

§ 11. Concerning these two factors of the "hull-efficiency," I need only say here that they appear to be to a great extent inter-dependent, that the conditions which increase the wake gain tend generally to increase the thrust-deduction loss, and that the product, the "hull-efficiency," is therefore, roughly speaking, constant. It is, in fact, roughly equal to unity in all cases, the value of the one factor being roughly equal to the reciprocal of the other.

§ 12. I say "roughly speaking," but the point of importance to us here is this, that such variations as do exist in the value of the "hull-efficiency," appear to mainly accompany differences in shape of hull or in position of screw in reference to it, rather than differences in size or proportion of screws with the same hull. Consequently, for the present I think we need not trouble ourselves about the effect of change of size or proportion of screws on the "hull-efficiency," and may be content with examining the effect of such change on the "screw-efficiency," namely on the efficiency which the ship's screw would possess if working in undisturbed water, and maintaining the thrust T at the speed V_1.

§ 13. The leading propositions concerning the efficiency of screws in undisturbed water, which I am going to put forward, are primarily based on the reasoning of the late Mr. Froude's Paper, read before this Institution in 1878, and entitled "On the Elementary Relation between Pitch, Slip, and Propulsive Efficiency." These propositions have been corroborated by the results of very numerous experiments on model screws made at Torquay; they are also corroborated, as I gather from Mr. Sydney Barnaby's work on "Marine Propellers,"* by the results of an entirely independent set of experiments therein referred to.

§ 14. The actual quantitative values embodied in the diagrams appended to this Paper, are obtained from a special set of experiments on model screws of different pitch-ratios, undertaken specially for the purposes of a Report to the Admiralty dealing with the same subject as this Paper. It is not easy to obtain perfectly trustworthy results from experiments on model screws, and the data in question are doubtless capable of correction; but they may be relied upon as quite correct enough for the practical uses to which I here propose to put them.

* The investigation dealt with in the present Paper was practically completed, and the Report on the subject to the Admiralty to a great extent shaped, before I saw Mr. Barnaby's work. Although this Paper necessarily covers some of the same ground, its scope is so far different that I have been unable to so arrange the subject as to explicitly utilise the work already done by Mr. Barnaby.

§ 15. The results of the set of experiments in question have been condensed into the diagram given in Fig. 4 (Plate XXVIII.). An explanation of the structure and purport of this diagram requires the following prefatory remarks, of which the first five may conveniently be put in the form of propositions.

§ 16. Prop. i.—The performance of any given screw advancing at any given speed through the water, and turning at various numbers of revolutions per minute (in other words, working with various degrees of slip-ratio), may be represented by a diagram such as that in Fig. 1 (Plate XXVII.), where the abscissæ indicate values of slip-ratio*; the ordinates of the curve B.B., the corresponding thrusts; and those of the curve A.A., the corresponding efficiencies.

Prop. ii.—With given slip-ratio, the thrust of a given screw varies as the square of the speed of advance through the water.

Prop. iii.—With given slip-ratio, and given speed of advance, and with given design of screw and varying size, the thrust varies as the square of the dimension of the screw.

Prop. iv.—With given slip-ratio and given design of screw, the efficiency is unaffected by variations of speed or of size of screw.

Prop. v.†—(Consequent upon the four preceding.) A single diagram, such as that in Fig. 1, will represent the performance of any number of screws of given design but of differing sizes, advancing at any variety of speeds through the water; if the ordinates of the thrust curve are taken to represent, not thrust simply, but values of the expression

$$\frac{T}{D^2 V_1^2}$$

where T = Thrust, D = Diameter of screw, and V_1 = Speed of advance through the water.

§ 17. Thus, a single diagram such as Fig. 1, may be made to express for any variety of conditions of operation, the performance of any number of screws of any size, but of

* This diagram represents the thrust as zero for zero of slip-ratio, as it would be in a true screw were there no water friction or other element of fore-and-aft resistance. This appears (from the model screw experiments) to be practically very nearly correct for screws of ordinary make (*i.e.*, with round-backed blades) since such screws have an effective pitch somewhat greater than their nominal pitch, and at zero of slip (by nominal pitch) have sufficient *effective* slip to counterbalance the fore-and-aft resistance.

† This proposition, and the four preceding on which it rests, are theoretically true; but for large differences of size and speed (as between models and full sized screws), they probably need a correction analogous to the "skin-friction correction" in the "law of comparison" of resistances of models and ships. [*See* below, §. 32.]

uniform design. The only differences of design which appear to me important enough to demand notice in this investigation, are two in number, viz. differences in number of blades, and differences in ratio of Pitch to Diameter (or "Pitch-Ratio").

§ 18. Dealing first with differences in number of blades; from several sets of experiments on model screws, I have found that the respective thrusts of four-bladed, three-bladed, and two-bladed screws, of the same diameter, pitch-ratio, and pattern of blade, and working at the same slip-ratio, are proportioned to one another as the values 1 : 0·865 : 0 65. (From this it will be seen that the thrust per blade with constant slip-ratio, decreases somewhat with increase of number of blades.) For the present, I shall treat the efficiency as independent of the number of blades; but will recur to that point later on.

§ 19. Next, as to the effect of differences in Pitch-Ratio. From the special experiments on model screws of various pitch-ratios, already referred to, it appeared that the several curves of the kind shown in Fig. 1, for screws of different pitch-ratios, if brought together on a common abscissa-scale of slip-ratio, would present a combined diagram such as is shown in Fig. 2 (Plate XXVII.), where the curves B_1, B_2, B_3, are the several successive thrust curves of a series of screws of successively increasing pitch-ratios, and A_1, A_2, A_3, the corresponding efficiency curves. The relations between the curves on such a diagram may be characterized by saying that the several thrust curves, and likewise the several efficiency curves, are (approximately) the same curve repeated on differenta bscissa-scales; the difference between the abscissa-scales of the thrust curve (or rather the curves of $\frac{T}{D^2V_1^2}$) being greater than those between the scales of the corresponding efficiency curves.

§ 20. This statement, implying as it does that there is no material difference in maximum efficiency between screws of different pitch-ratios, will probably cause surprise. At any rate, I was myself surprised to find that within so large a range of pitch-ratio as was included in the experiments (viz., from 1·2 to 2·2), there was so little, if any, difference in maximum efficiency. The theoretical investigation by the late Mr. Froude, already referred to, gives 45° as the most efficient mean angle of blade to axis, and this, if the mean effective diameter be taken as $\frac{2}{3}$ the extreme diameter, implies a pitch-ratio of about = 2·0. The results of the model experiments I have referred to appear to show a slightly growing efficiency with increasing pitch-ratio, within the limits of experiment; a result which, so far, is fairly consistent with this theoretical proposition; but the differences were so slight as to be possibly attributable to accidental causes. I therefore consider that, for

practical purposes, we may regard the maximum efficiency as unaffected by pitch-ratio variation of pitch-ratio, at any rate with pitch-ratios ranging between 1·2 and 2·4, or possibly even a somewhat wider range.

§ 21. Let us now proceed to examine in what way information concerning the performance of screws, presented in the form we have been considering, naturally applies to the problem of the best size of screw for given conditions.

§ 22. We have just seen that, on the score of efficiency, no particular pitch-ratio (within broad limits) is to be preferred to any other, and we know from experience that different classes of ships demand different pitch-ratios in order to suit the revolutions per minute to the I.H.P. of the engines. Further, in each individual ship, since the desired revolutions per minute for full speed may be regarded as a fixed quantity, dependent on the I.H.P., to be developed, any variation of diameter involves a concomitant variation of pitch-ratio; hence in calculating the dimensions for screws, the diameter and pitch-ratio must alike be regarded at starting as unknown quantities, neither of which can be determined before the other.

§ 23. Nevertheless, it is instructive to first study the problem in the simpler form which it presents, if we suppose that by some invariable rule screws were always made of one and the same pitch-ratio; let us suppose also that for such invariable pitch-ratio a diagram of the nature of Fig. 1 has been accurately determined.

On this supposition the problem is simple enough. The efficiency is then connoted directly by the slip-ratio, and each degree of slip-ratio corresponds to a certain value of $\frac{T}{D^2 V_1^2}$, indicated by the thrust curve. To determine the diameter of screw to suit any given values of T and V_1, we have simply to find the value for D which gives to $\frac{T}{D^2 V_1}$ the value corresponding to the slip-ratio of maximum efficiency. Or, if such diameter should prove impracticably or inconveniently large, we are in a position to judge by the efficiency curve how much we can afford to diminish D, and consequently increase $\frac{T}{D^2 V_1^2}$ and slip-ratio, without unwarrantable sacrifice of efficiency. Were it not, indeed, that circumstances frequently demand such a compromise as this, or did the efficiency curve much more markedly accentuate the point of maximum efficiency, and thereby more strictly limit the range of slip-ratio within which nearly equally good efficiency was obtainable, it would be doubtless sufficient to specify once for all the slip-ratio and value of $\frac{T}{D^2 V_1}$ corresponding to the maximum efficiency. But as things stand this would be a very imperfect

treatment of the problem. What we need is to keep the form of the efficiency curve clearly in view, and to have means of finding the position in its abscissa-scale appropriate to any suggested dimensions of screw and values of thrust and speed.

§ 24. This position in the abscissa-scale of the efficiency curve is the token, so to speak, of the standing of the screw in reference to efficiency, and in our supposititious case of invariable pitch-ratio the criterion of this abscissa position is ready to hand in the shape of the slip-ratio, or its attendant value of $\frac{T}{D^2 V_1^2}$. But, as is evident from the nature of the diagram shown in Fig. 2 (Plate XXVII.), neither of these characteristics will serve as a common criterion of abscissa position in reference to the efficiency curve, for screws of different pitch-ratios, nor does any other natural characteristic present itself to serve the purpose. In default, then, of any suitable natural characteristic, our only resource seems to be to introduce an artificial one, and this I have done by the expedient of arbitrarily, or rather empirically, modifying the several abscissa-scales of the diagrams for the successive different pitch-ratios of screw, (such as are shown in Fig. 2,) so as to bring their several efficiency curves into coincidence. The combined diagram thus produced is illustrated in Fig. 3. The reading on the arbitrary abscissa-scale to which it is plotted, and which shows alike for screws of different pitch-ratios, the position in the abscissa-scale of the common efficiency curve, I have termed for want of a more descriptive title, the "abscissa-value."

§ 25. The dotted lines indicating successive degrees of slip-ratio, which in Fig. 2 were vertical, are retained in Fig. 3; but in virtue of the modification of abscissa-scale to which the diagrams have been subjected, these lines have in Fig. 3 become curved and slanting. The function of these curves is to specify the slip-ratio proper to the points at which they intersect the thrust curves for the several pitch-ratio values, and they thus enable the revolutions per minute proper to these points of intersection to be determined for any given diameter and speed of advance of screw.

§ 26. Supposing, then, a diagram of the nature of Fig. 3 (Plate XXVII.) to have been determined with sufficient accuracy, and to include curves for sufficiently close gradations of pitch-ratio, it will be seen that it would afford all the materials necessary for determining for any given diameter, pitch-ratio (within limits), and values of V_1 and T, the abscissa-value, and also the revolutions per minute with which the screw would work. It would accordingly enable us for any assigned values of V_1 and T, and revolutions per minute, in the first place

to select the dimensions of screw for maximum efficiency, and secondly to form an opinion as to what modifications of these dimensions might be admissible, to suit structural or other accidental conditions.

§ 27. Fig. 4 (Plate XXVIII.) is a diagram of this kind, deduced from the experiments on model screws of different pitch-ratio, already referred to. I will presently show how the information may be converted into a form more convenient for practical use ; but before doing so, it is necessary (1) to explain more in detail the process by which the experimental results have been reduced into the form of the diagram in Fig. 4 ; and (2) to examine to what extent, if at all, this diagram, obtained as it is purely from experiments on model screws, needs to be discounted or corrected for application to full-sized screws propelling actual ships.

§ 28. The model screws experimented on were of a uniform diameter (0·68 ft.) and of four different pitch-ratios, viz., 1·225, 1·4, 1·8 and 2·2. In order to be able to satisfactorily interpolate results for closer gradations of pitch-ratio, between the results given by these screws, and moreover to ensure that the diagram should throughout not only present absolute quantities with sufficient correctness, but also correctly exhibit the nature of the differences in result attendant on minor differences in pitch-ratio, it was necessary to eradicate the minor irregularities and inconsistencies presented by the results, and which were presumably due to minor errors in the experiments. This was done, in the process of reduction into the form of the diagram in Fig. 4, as follows: An analysis of the results showed (*a*) that the relations between the thrust curves of the several screws could be expressed empirically (within the limits of error of the results) by the proposition that with given slip-ratio, the thrust (or value of $\frac{T}{D^2\ V_1^2}$) varies as the reciprocal of the pitch-ratio, *minus* a constant, the value of this constant being 0·17.* It also showed (*b*) that the thrust (or value of $\frac{T}{D^2\ V_1^2}$) for constant efficiency might be taken to vary inversely as the power 0·8 of the pitch-ratio. Thrust curves for the several screws were then drawn so as to most nearly agree with the actual results of experiment, while consisting with proposition (*a*), and by means of the same proposition thrust curves were deduced from these for intermediate gradations of pitch-ratio, as well as for other gradations extending

* This empirically held good throughout the range of variation of pitch-ratio (1·225 to 2·2) dealt with in the experiments. Of course it could not hold good for unlimited range of pitch-ratio, or it would imply that the thrust of a screw of pitch-ratio $= 5{\cdot}9$ (*i.e.* $=\frac{1}{0{\cdot}17}$) would $= 0$ at all slip-ratios.

somewhat beyond the limits of 1·225 and 2·2 covered by experiment. The abscissa-scales were then so modified that the ordinates of the several thrust curves at any common abscissa-value should be proportioned to one another according to proposition (*b*). In performing this latter process the abscissa-scale of one of the curves, viz., that for 2·2 pitch-ratio, was retained unaltered; consequently for this pitch-ratio, "abscissa-value" is exactly proportioned to slip-ratio.*

§ 29. The thrust curves of the diagram were plotted on such an ordinate scale as to show, not actual thrust for the diameter and speed of experiment of the model screws, but value of $\frac{T}{D^2 V_1^2}$; where T = thrust in tons, D = diameter in tens of feet, and V_1 = speed in tens of knots. The curves may therefore be equally regarded as showing absolute thrust for a screw of a unit diameter of 10 feet, at a unit speed of 10 knots.

§ 30. The model screws were four-bladed, and the diagram is primarily correct for four-bladed screws. It is made applicable to two-bladed or three-bladed screws, in virtue of the data given above in § 18, by means of special scales, appropriate to the number of blades, for interpreting the ordinates of the thrust curves.

§ 31. The revolutions per minute appropriate to each of the intersections of the thrust and slip-ratio curves were calculated for the unit diameter of 10 ft., and the unit speed of 10 knots, and plotted as ordinates at the abscissa-values of the respective intersections. By means of the spots so obtained curves were drawn in, showing for the successive pitch-ratios to which they are appropriate revolutions per minute for varying abscissa-value. The ordinates of these curves show absolute revolutions per minute for the unit diameter of 10 ft., and unit speed of 10 knots; but since, with constant pitch-ratio and slip-ratio, revolutions per minute vary directly as speed and inversely as diameter, they may be equally regarded as showing values of the expression $\frac{R\ D}{V_1}$, where R = revolutions per minute, D = diameter in tens of feet, and V_1 = speed in tens of knots.

Next, as to the applicability of this diagram to full-sized screws, the points we have to consider are the following:—

§ 32. Although the terms in which the diagram is expressed make it nominally appli-

* The unit of the "abscissa-scale" count (chosen arbitrarily for convenience in reading off the diagram as originally plotted corresponded to a slip-ratio of 2·5 per cent., so that for a 2·2 pitch-ratio screw the "abscissa-value" is $\frac{1}{2{\cdot}5}$ of the slip per cent.

cable to screws of any size and travelling at any speed, yet, strictly speaking, the diagram merely expresses the results of experiments on model screws of ·68 ft. diameter, travelling at a speed of 206 ft. per minute. It is made applicable to any size and speed by virtue of Propositions ii. and iii. given above in § 16. Now these propositions are theoretically true, on the assumption that the skin-friction resistance of the screw-blades varies strictly as the area of surface and as the square of the speed; consequently, in so far as these assumptions may be inaccurate, the diagram presumably needs, for full-sized screws, a correction analogous to the "skin-friction correction" which has to be introduced into the "law of comparison" in calculating the resistance of a ship from that of her model. But for accurately determining the amount of such correction for screw propellers, we have at present no satisfactory data.

§ 33. Again, the diagram, assuming it to be correct for full-sized screws, serves to determine the best diameter of screw for given values of T (*i.e.*, thrust), and speed V_1.* But commonly, when we wish to determine the best dimensions of screws for any actual ship, the values from which we have to work are not these, but are the resistance (or E.H.P.), and speed V.† Now, the "thrust-deduction percentage," or percentage by which resistance is less than thrust, and the "wake-percentage," or percentage by which speed V exceeds speed V_1, have been determined for many models of ships by means of experiments on those models with model-screws; and for models on which such experiments have not been made, these values may be pretty accurately inferred from the results of experiments on models of analogous forms. We can, therefore, generally convert the values of resistance and speed V for any given ship, into values of thrust and speed V_1, if we assume that the "thrust-deduction" and "wake" percentages will be the same for ship as for model. But there are grounds for supposing that these percentages will both be somewhat less in ship than in model, in virtue of the (relatively) lessened surface friction of the ship; so that here again is an element of uncertainty.

§ 34. In the effects referred to in the two preceding paragraphs, we have to do with two principal sources of inaccuracy; viz. (1), inaccuracy of the diagram itself, as applied to full-sized screws; (2) inaccuracy in the estimate of the values of T and V_1 for a full-sized ship, by which the diagram is to be applied to each individual case. Now according to the reasoning of the late Mr. Froude's Paper of 1878, the diagram, being based on model screw experiments, and thereby in effect exaggerating the skin-friction

* † *See* above, § § 6 to 10.

co-efficient of the screw blades, must place the maximum point of the efficiency curve at too high an abscissa-value, and in effect exaggerate the abscissa-scale of the curve. On the other hand, attributing to the ship the full model values of "wake" and "thrust-deduction" tends to underrate V_1 and overrate T, and thus exaggerate the abscissa-value appropriate to a given screw propelling a given ship at given speed. Hence, in the use of the diagram for what we may regard as its primary purpose of determining how a given ship's screw stands in reference to efficiency, as betokened by the position appropriate to it in the abscissa-scale of the efficiency curve, the two principal sources of inaccuracy tend to counteract each other. In a very similar way, the two sources of inaccuracy apparently tend to counteract each other also in the use of the diagram for what may be described as its secondary purpose, viz., of determining the revolutions per minute with which the screw will work.

§ 35. In regard to both the purposes of the diagram, we have therefore to deal not with a single error, nor with several errors tending in one direction, but with a balance between conflicting errors; hence, even though either source of error taken alone might be important, it is quite possible that the diagram may with sufficient accuracy be used as proposed for full-sized ships. How far this is so may be in some degree tested by comparisons of the results of steam trials of ships with results assigned by the diagram as appropriate to the conditions attending those trials.

§ 36. With a view of testing in this way the sufficient correctness of the diagram as used for what I have termed its secondary purpose, I have compared the revolutions per minute recorded in the steam trials of a variety of ships of which models have been tried at Torquay, with the revolutions per minute assigned by the diagram to these ships at the recorded speeds, and with the recorded dimensions of propellers. For our present purposes it is sufficient to state that the calculations from the diagram were found on the average to overrate the revolutions per minute of twin-screws, and to underrate those of single screws, by about two or three per cent.

§ 37. Fig. 5 (Plate XXVII.) shows the application of a similar test in reference to the primary purpose of the diagram, viz., the determination, for a given ship, speed, and screw, of the standing of the screw in respect of efficiency. Here, the "propulsive co-efficients," or values of $\frac{\text{E.H.P.}}{\text{I.H.P.}}$, given by the trials of various ships, are plotted as ordinates to the abscissa-values determined by the diagram, Fig. 4, for the several ships, speeds and

dimensions of screws. These plottings are shown in comparison with the efficiency curve deduced from the model screw experiments and shown in Fig. 4.*

§ 38. The spots show very markedly the decline of efficiency near the zero end of the abscissa-scale; and if we confine our attention to those results which are most strictly comparable with one another, viz., those of the successive trials of the "Iris," we find the spots falling into a line, bearing great resemblance to the corresponding portions of the model screw efficiency curve. So regarded, these results exhibit the differences in propulsive efficiency between the successive "Iris" trials, as presumably the legitimate outcome of the known differences in condition of operation of propeller which are here expressed by the differences in "abscissa-value."

§ 39. The rest of the spots fall for the most part very irregularly, much too much so to afford data for assigning any definite correction to the efficiency curve of the model screws, or even for determining confidently whether or not it requires correction at all. These irregularities in the plottings undoubtedly witness to the existence of some material factors of performance which are not duly taken account of either by the resistance experiments on the models of these ships, or by the model screw experiments. That these unknown factors are in great part accidental, so to speak, is witnessed by the differences between the "Thunderer" and "Devastation" results; but even assuming them to be genuine effects of difference in design of hull, or design or position of screws, their existence would not invalidate the general conclusions as to influence of size of screws upon efficiency, which have been deduced from the model screw experiments, and embodied in Fig. 4. They only concern us here, in so far as they practically defeat the attempt to accurately determine how much correction those conclusions may require for application to full-sized screws.

§ 40. The position then, may be stated thus: We may rely, as I consider—(1) on the model screw efficiency curve as sufficiently correct in character; (2) on the abscissa-values calculated for screws of full-sized ships from Fig. 4 in the manner which has been specified, as sufficiently correct, relatively to one another; but (3) perhaps erroneous by some uncertain amount relatively to the true efficiency curve. Now, this uncertainty no doubt weakens, but does not by any means destroy, the value of the information, when the practical purposes for which it is needed are duly considered.

§ 41. The information is needed to supply the answers to two questions; viz. (1), what size of screw should, or at any rate may, be chosen, if efficiency is the only consideration; (2) how much may the size so chosen be departed from in case of need.

* This diagram shows propulsive-coefficients of twin-screw ships only. Results of some single-screw ships were similarly plotted; but they extended over so small a range of abscissa-value, and were so conflicting, as to be uninstructive.

§ 42. Now considering the character of the efficiency curve and the great range of abscissa-value within which the efficiency is practically constant, the answer to the first question is plainly not a matter of great nicety. It is quite true that we cannot determine with certainty the abscissa-value of absolute maximum efficiency; but there is no occasion for so doing. All that we really need is to determine on a value which will ensure a performance not substantially inferior to the best, and this we are certainly in a position to do. Probably any value between say 8·0 and 11·0 would be a safe choice; but as will presently be seen, for convenience in manipulating the data, there are advantages in adopting one particular standard value, and I have accordingly selected the value 9·0 for this purpose.

§ 43. To the second question, the uncertainty of the data no doubt does preclude a very definite answer. Supposing it to be desirable on grounds of convenience, in any given case, to use a much smaller diameter than that proper to the 9·0 abscissa-value just chosen, we should wish to be in a position to estimate accurately the loss of efficiency which any suggested degree of reduction of diameter would entail, in order that we might judge at what point in the process of reduction the price paid would begin to outweigh the advantages sought. This, unfortunately, we are not at present in a position to do with confidence. All we can say is that, judging from the "Iris" results,which are the most harmonious, and also for other reasons, perhaps, the most trustworthy, of those in Fig. 5, we should infer that the efficiency curve deduced from model screws is in effect too close, rather than too open, in abscissa-scale. If so, an implicit use of the data may be expected to overrate rather than underrate the penalty attaching to excess of abscissa-value; and we may on the whole expect to be on the safe side as far as efficiency is concerned, if we guide ourselves by the model-screw curve as it stands.

I will now refer to the form into which the information embodied in Fig. 4 has been reduced with a view to greater convenience in using it.

§ 44. The purpose of the information is to enable us to select the dimensions of propellers most suitable to any new design of ship. We must, of course, suppose the speed to be fixed on, and the necessary I.H.P. to be determined. In order for the engines to develop this I.H.P. most advantageously they should run at a certain number of revolutions. We may therefore regard the speed, I.H.P., and revolutions per minute, as the fixed quantities from which we are to work. For these quantities we wish first to determine the size of propeller which will give the 9·0 abscissa-value, and then, should such size be inconveniently large, to determine what change of abscissa-value will accompany what

modification of diameter, and likewise what concomitant modification of pitch-ratio is necessary to conserve the revolutions per minute unaltered.

§ 45. For this purpose we do not require a diagram constructed so as, like Fig. 4, to give information directly for any abscissa-value. The diagrams will, on the contrary, be more convenient to use (as well as less complicated to arrange) if contrived so as to show (1) the dimensions of screw for the 9·0 abscissa-value simply; (2) the modifications of diameter and pitch-ratio which attend the change from the 9.0 value to any other. Diagrams prepared with this view will be found in Figs. 6 to 9 (Plates XXIX., XXVII. and XXX.) These diagrams are arranged so as to determine the diameter and pitch-ratio for any given revolutions per minute, I.H.P., and speed of ship, without requiring these values to be converted into thrust and speed V_1. In putting the data in this form, it has, of course, been necessary to make certain assumptions as to the value of the propulsive co-efficient, thrust-deduction, and wake, but particulars are given to enable corrections to be made, if required, for any want of correctness of these assumptions in any particular case. How the information has been brought into this form may be understood from the following reasoning.

§ 46. In determining the dimensions of screw for the abscissa-value 9·0, we have to deal with constant abscissa-value. Let us first assume constant pitch-ratio also. Now constant abscissa-value and pitch-ratio, mean constant slip-ratio; whence

$$T \propto D^2 V_1^2$$

$$\therefore T V_1 \text{ (or T. H. P.)} \propto D^2 V_1^3.$$

For the degree of accuracy here required* we may take hull-efficiency as constant, in fact as = unity (see § 11), so that E. H. P. = T. H. P.

$$\text{Let } \frac{\text{E. H. P.}}{\text{I. H. P.}} \text{ (\textit{i.e.} propulsive co-efficient)} = p.$$

$$\text{Then } p \times \text{I. H. P.} \propto D^2 V_1^3$$

Now pitch-ratio and slip-ratio being both constant,

$$\text{Revolutions per minute} \propto \frac{V_1}{D}$$

* See § 51 (third sentence).

Thus if we also assume revolutions per minute = constant,

$$V_1 \propto D.$$
$$\therefore p \times \text{I. H. P.} \propto D^5 \propto V_1^5.$$
$$\text{Let } \frac{V}{V_1} = 1 + w^*$$
$$\text{Then } V_1^5 = \left(\frac{V}{1+w}\right)^5$$

If then we assume certain constant standard values for p the propulsive co-efficient, and $1 + w$ the wake factor, the values

$$\frac{I^{\frac{1}{5}}}{V} \quad \text{and} \quad \frac{D}{I^{\frac{1}{5}}}$$

become constant.

That is to say, for the general conditions with which we have here to do, viz., for the abscissa-value 9·0, and for constant standard values for propulsive co-efficient and wake, the above values are constant for given pitch-ratio and revolutions per minute.

§ 47. These values have been worked out by means of the diagram in Fig. 4†, for various values of pitch-ratio and revolutions per minute, and are shown in Fig. 6 as the ordinates of curves successively appropriate to successive numbers of revolutions per minute, the curves being plotted on an abscissa-scale representing pitch-ratio.

* Then $100 \times w$ = what is termed the "wake per-centage," viz., the per-centage of V_1 by which V exceeds it (see § 33).

† The ordinates of these curves were actually obtained from a diagram embodying the same data as Fig. 4, in a somewhat different form. They could be obtained from Fig. 4 as follows :—

Let the ordinates of the thrust and revolution curves on Fig. 4 for any given pitch-ratio, at the 9·0 abscissa-value, $= t$ and r respectively. Then

$$t = \frac{T}{D^2 V_1^2}; \quad r = \frac{R\,D}{V_1}$$

Hence we readily get

$$\frac{T\,V_1}{V_1^5} = \frac{t\,r^2}{R^2}; \text{ and } \frac{D^5}{T\,V_1} = \frac{r^3}{t\,R^3}$$

$$\therefore \quad \frac{I^{\frac{1}{5}}}{V} = k_1 \times \sqrt[5]{\frac{t\,r^2}{R^2}}; \text{ and } \frac{D}{I^{\frac{1}{5}}} = k \times \sqrt{\frac{r^3}{t\,R^3}}$$

[Where k_1 k_2, are constants, involving the assumed standard ratio of $T\,V_1$ to I. H. P., k_1 involving also the assumed standard ratio of V to V_1.]

These values for $\frac{I^{\frac{1}{5}}}{V}$, and $\frac{D}{I^{\frac{1}{5}}}$ are then the ordinates for the curve (in Fig. 6) appropriate to the revolutions per minute R, at the pitch-ratio which gives the values t & r

The curves given in Fig. 7 (Plate XXIX.) enable I.H.P.* to be readily converted into $I^{\frac{1}{5}}$ without the use of Tables.

§ 48. The complete process of determining the required dimensions of screw for given I.H.P., speed of ship, and revolutions per minute, by means of these diagrams, may be detailed as follows:

(1.) Convert I.H.P. into $I^{\frac{1}{5}}$ by means of the curves in Fig. 7.

(2.) Divide $I^{\frac{1}{5}}$ by V.

(3.) Select the curve of $\frac{I^{\frac{1}{5}}}{V}$ appropriate to the intended revolutions per minute.

(4.) Find the point in its abscissa-scale at which it has the ordinate value $\frac{I^{\frac{2}{5}}}{V}$ already found. This point denotes the required pitch-ratio.

(5.) Read off the ordinate of the curve of $\frac{D}{I^{\frac{1}{5}}}$ for the same number of revolutions per minute at the same abscissa position.

(6.) Multiply this ordinate value by $I^{\frac{1}{5}}$, and the product is the required diameter.

The curves in Fig. 6 (Plate XXIX.), as already explained, assume certain constant standard values for $(1+w)$ and (p), viz., the symbols which take account of wake and propulsive co-efficient. To correct, if necessary, for deviations from these assumed standard values, is a very simple process.

§ 49. Let us term these standard values $1 + w_1$ and p_1. Then the symbols, V and I, in the expressions, $\frac{I^{\frac{1}{5}}}{V}$ and $\frac{D}{I^{\frac{1}{5}}}$, stand respectively for $V_1 \times (1 + w_1)$ and E.H.P. $\times p_1$. If we term the actual values $1 + w$ and p, the actual speed and I.H.P. may be termed $V_1 \times (1 + w)$ and E.H.P. $\times p$. Consequently, to convert the actual speed

* The curves in Fig. 6 are for a four-bladed screw. To apply them to a two-bladed or three-bladed screw the intended I.H.P should first be increased in the ratio in which the thrust of a four-bladed screw exceeds that of a two-bladed or three-bladed screw of the same diameter, working with the same slip-ratio and at the same speed (see § 18). To supersede this arithmetical operation two additional curves are given in Fig. 7, shewing the values of $I^{\frac{1}{5}}$ that should be used for two-bladed and three-bladed screws, plotted to the common abscissa-scale representing actual intended I.H.P.

and I.H.P. into the values which should be taken for these quantities for use in the diagram, the former should be respectively multiplied by the values $\frac{1+w_1}{1+w}$ and $\frac{p_1}{p}$.

§ 50. The value of $(1+w)$ depends mainly upon form of hull and position of screw or screws in reference to it. As the readiest means of prescribing the correction which should be made for the difference which in any given case may exist between $1+w$ and $1+w_1$, I have given in Fig. 8 (Plate XXVII.) a scale shewing the amounts of this correction for various existing ships, as determined by screw experiments on the models of those ships. For any new design the correction may be inferred, by means of this scale, from the corrections therein given for such existing designs as the new design most nearly resembles. The readings on this scale denote, not values of $(1+w)$, but values of the multiplier $(\frac{1+w}{1+w_1})$, (= "W," say), by which the expression $\frac{I^{\frac{1}{5}}}{V}$ has to be multiplied in order to effect the correction. Thus, strictly speaking, the ordinates of the curves in Fig. 6 shew not values of $\frac{I^{\frac{1}{5}}}{V}$, but values of $\frac{I^{\frac{1}{5}}}{V} \times W$, the value of W being = unity whenever the standard value $1+w_1$ may be deemed sufficiently correct.*

§ 51. The assumed standard value $p_1 = 0{\cdot}5$, in other words it has been assumed that $\frac{\text{E.H.P.}}{\text{I.H.P.}} = \frac{50}{100}$. I consider that in practice it would be seldom necessary to introduce the correction for deviation from this value. It must not be forgotten that, although in the course of this investigation we are continually dealing with figures of I.H.P. and propulsive co-efficients, and the like; yet in doing so, we are not determining the I.H.P which the ship will require; we are merely using the estimated I.H.P. as a measure of the requisite dimensions of screws, and for this purpose it need not be very accurately determined.

§ 52. It may be asked, what has become of the allowance for "thrust-deduction" which was represented in § 33 as essential to the application of the data embodied in Fig. 4, to screws propelling ships. The answer is that in the method of presenting the data here adopted, the allowance for thrust-deduction is in effect included in that for "wake," in virtue of the assumption that "hull efficiency" = unity (*see* § 46, *see also* footnote to § 50). Hence, in correcting for deviations from the assumptions used in calculating the

* This standard value $1+w_1 = \frac{1}{0{\cdot}9}$, this being the value for $1+w$, which corresponds, for hull efficiency = 1, to a value of $\frac{\rho}{T}$ (see § 9) of 0·9. The standard wake percentage is, in fact, that corresponding to a 10 per cent. thrust-deduction.

diagram in Fig. 6, there is no occasion to make explicitly any correction for variations in "thrust-deduction."

§ 53. Fig. 9 shows the modification of diameter and pitch-ratio which must attend the change from the 9·0 abscissa-value to any other; I.H.P., speed, and revs. per min. remaining constant. The ordinates of the two groups of curves respectively denote values of $\frac{D}{D_1}$ and $\frac{P}{P_1}$; where D_1 and P_1 are the diameters and pitch-ratio for the 9·0 value, and D and P those for the abscissa-values to which the change is made. The successive curves of each group are proper to successive convenient values of P_1.* The curves for the several values of P_1 differ so little, that in Fig. 10 (Plate XXX.) I have shown by themselves the curves for $P_1 = 1{\cdot}2$, as representing the rest with sufficient accuracy for use in ordinary cases.

§ 54. The primary purpose of the information contained in this paper, as the title implies, is to enable the dimensions for screws to be judiciously selected to suit novel conditions. But by a converse process the information may sometimes usefully serve to explain any abnormal results of steam trials, such as an unexpected failure in speed.

* The ordinates of these curves, like those of the curves in Fig. 6 (*see* footnote to § 47), were actually obtained from a diagram embodying the same data as Fig. 4, in a different form. They could be obtained from Fig. 4 as follows:—Let the ordinates of the thrust and rev. curves for pitch-ratio P_1, at the 9·0 abscissa-value, $= t_1$ and r_1 respectively. Let the diameter for this abscissa-value and pitch-ratio P_1, $= D_1$, and let V, R, and T, be the speed, revs. per min., and thrust. These last three values are to remain constant while the abscissa-value changes.

$$\text{Then } t_1 = \frac{T}{D_1^2 V^2}; \quad r_1 = \frac{R D_1}{V}$$

If we change the pitch-ratio from P_1 to P_n, the diameter will change from D_1 to some unknown diam. $= D_n$, and the abscissa-value from 9·0 to some unknown value $= A_n$. Let the (at present unknown) ordinates of the thrust and rev. curves for pitch-ratio P_n at the abscissa-value A_n, $= t_n$ and r_n respectively.

$$\text{Then } t_n = \frac{T}{D_n^2 V^2}; \; r_n = \frac{R D_n}{V}$$

$$\text{Hence } \frac{t_n}{t_1} = \left(\frac{D_1}{D_n}\right)^2; \; \frac{r_n}{r_1} = \frac{D_n}{D_1}$$

$$\therefore \frac{t_n}{t_1} = \left(\frac{r_1}{r_n}\right)^2$$

We have then to find from the diagram what is the actual value of A_n which makes $\frac{t_n}{t_1} = \left(\frac{r_1}{r_n}\right)^2$, and in doing so we also find the absolute value of $\frac{r_n}{r_1}$. This value $= \frac{D_n}{D_1}$, viz., the ordinate for the curve showing $\frac{D}{D_1}$, for the abscissa-value A_n. The ratio $\frac{P_n}{P_1}$ is the corresponding ordinate of the curve showing $\frac{P}{P_1}$.

§ 55. The steam trial records give values for speed, I.H.P., and revolutions per min. From these values, by means of the curves in Fig. 6, the pitch-ratio and diameter proper to the 9·0 abscissa-value may be found; and the ratio of the actual diameter to the diameter so found, determines by aid of the curves in Fig. 9 the probable actual abscissa-value for the screws of the ship under the circumstances of the steam trial. It will then be at once apparent whether the supposed failure of performance is attributable to any abnormal abscissa-value, or in other words to any abnormal relation between the work actually demanded of the screws and that of which they are efficiently capable.

§ 56. Again, the abscissa-value for the recorded speed, I.H.P., revolutions per min., and diameter of screw, having been thus found, the curves will likewise give a value for the pitch-ratio proper to these conditions. If this value agrees fairly with the recorded actual pitch-ratio, it will be an evidence that the records of the trials are at least consistent with themselves, and that so far as the action of the propeller is concerned there is no markedly irregular circumstance, to which the defective performance might be attributed, (as *e.g.* abnormal wake or thrust-deduction, or deformed screw blades), which if it existed could hardly fail to betray itself by disagreement between the calculated and actual pitch-ratios. In such a case the natural inference would be that the defective performance was due to a genuine excess of resistance of the ship. On the other hand, should a conspicuous disagreement between the calculated and actual pitch-ratio, witness to the existence of some such irregular circumstance, a consideration of the various alternative hypotheses by which the facts can imaginably be reconciled, may afford some clue to its nature.

§ 57. For such analysis, the fundamental diagrams in Fig. 4 will probably be more generally serviceable than the diagrams in Fig. 6, derived from them; but in some cases the diagram in Fig. 11 (Plate XXXI.) will be found most convenient. Generally, the element which it is desirable to first evaluate is the true slip-ratio, since when this is determined, the true pitch-ratio and revolutions per min. being known, we obtain at once a direct measure of the speed V_1, and thence, assuming the correctness of the recorded speed V, a measure of the "wake" value: or conversely, if we should be in a position to confidently assign (within limits) a value to the "wake," we should obtain a check on the recorded speed V. Now in order to use Fig. 4, it is necessary first to assign a value to speed V^1, which is the very quantity we here wish to determine: on the other hand, the curves in Fig. 11 enable the slip-ratio to be determined for a screw of any given diameter and pitch-ratio, solely from the thrust and revolutions per min.

§ 58. We know that for a screw of given pitch-ratio and number of blades and working with given slip-ratio,

$$T \propto D^2 V_1^2$$
$$\text{also } V_1 \propto D R$$
$$\therefore T \propto D^4 R^2 \quad *$$

The ratio $T : D^4 R^2$ will vary with both pitch-ratio and slip-ratio, and the values of it are shewn in Fig. 11 as ordinates to curves appropriate to various pitch-ratio values plotted on an abscissa-scale of slip-ratio.

Extra scales are given, to adapt the diagram to two and three-bladed screws.

From these curves the slip-ratio is readily found for any given pitch-ratio and value of $\frac{T}{D^4 R^2}$.

[The ordinates of this diagram may of course be regarded as showing simply thrust for a screw of unit diameter and unit revolutions per minute, instead of values of $\frac{T}{D^4 R^2}$ for any screw.]

In concluding this Paper I must touch upon a few points which, in order to simplify the conceptions which I have been endeavouring to make clear, I have purposely left unnoticed.

* From this equation we also get—

$$TV_1, (= \text{T.H.P.}) \propto D^5 R^3.$$

and since $\frac{\text{T.H.P.}}{\text{I.H.P.}}$, viz. the "screw-efficiency" as modified by the engine-efficiency, should be practically constant (barring any exceptional engine friction or extreme abscissa-value), we thus get

$$\text{I.H.P.} \propto D^5 R^3.$$

Thus, it would at first sight appear that a diagram shewing values of $\frac{\text{I.H.P.}}{D^5 R^3}$ would be more serviceable (as avoiding the necessity of determining the thrust) than that in Fig. 11. It will be found, however, that these values will become constant at large slip-ratio, and therefore fail to determine the slip-value. The sense of this result becomes readily apparent if we consider the case of a given screw working with given revolutions per minute, the slip being varied by varying the speed of progress V_1. As the speed decreases (and slip increases) the thrust continues to increase, and its value consequently continues to determine the slip, even (at least theoretically) up to 100 per cent. slip. But the value TV_1, in virtue of the diminution of V_1, ceases to increase (and then commences to diminish) at a much earlier point.

The value $\frac{\text{I.H.P.}}{D^5 R^3}$ does not appear to actually cease increasing (and therefore actually fail to determine the slip) until 40 per cent or 50 per cent. slip, but the determination of course becomes very weak considerably short of this point. I have therefore, in this Paper, discarded it altogether in favour of the (albeit) less convenient form $\frac{T}{D^4 R^3}$.

§ 59. In the first place, I have treated the subject (*see* § 12) as if variation of diameter of screw in a given ship affected the performance only in virtue of its influence on "Screw-efficiency," and that it did not affect the "hull-efficiency." This is not quite the case. According to the model and screw experiments, diminution of screw diameter, with single screws at least, generally increases hull-efficiency by a tangible though not very serious amount; and this fact, so far as it goes, is an additional argument in favour of small diameter *cæteris paribus*. This statement applies to twin screws also; for, although with twin screws the above-mentioned influence of variation of diameter on the hull-efficiency does not appear to exist, or but slightly, we have in place of it the consideration that reduction of diameter of twin screws admits of reduction of resistance of shaft-tubes and fittings.

§ 60. Next, I have provisionally but incorrectly treated the maximum efficiencies of two-bladed, three-bladed, and four-bladed screws as equal. If this were truly so, the considerations referred to in the preceding paragraph would enjoin the invariable use of four-bladed screws in preference to others, on the ground that they have a smaller diameter for given abscissa-value. But as a fact, our experiments on model screws, and theoretical reasoning, alike lead to the conclusion that increase of number of blades tends to rather diminish the maximum efficiency; and I am, therefore, inclined to think that in virtue of these two opposing tendencies there is not much to choose between two, three, and four-bladed screws, so long, at any rate, as circumstances admit of the diameter proper to the 9 0 abscissa-value with either kind of screw. Where, on the other hand, limitation of diameter necessitates in any case an excessive abscissa-value, the lessened abscissa-value obtainable with a four-bladed screw of course turns the scale in its favour.

§ 61. The effect of difference in proportionate width of blade, to which I have not hitherto alluded, is, of course, similar to that of difference in number of blades, viz., difference in the diameter of screw appropriate to a given abscissa-value for given thrust and speed. Considering the character of the efficiency curve, and the large differences in abscissa-value necessary to cause a material difference in screw efficiency, it has appeared to me a needless refinement to attempt to take account of such small differences in proportionate width of blade as are commonly met with. Nor does it appear to me that any material differences in width proportion are demanded by the differences in the conditions in different ships. Even in the case just now supposed, of excessive abscissa-value entailed by limitation of diameter, I think it would be a mistake to attempt to reduce this excess by giving very unusual width of blade; on the ground that a degree of widening sufficient to do any

material good in this way would, as I consider, cause a loss of efficiency, owing to the disproportion of the blades, and of blade area to disc area, which would outweigh the *primâ facie* gain due to the reduced abscissa-value.

§ 62. Lastly, I have implied that, in determining the best size and proportions of screw, the revolutions per minute may be selected solely with reference to the intended H.P. of the engines, and the pitch-ratio allowed to settle itself accordingly. There are, of course, limits to the proper application of this method, and cases may arise where following it implicitly would assign a pitch-ratio outside of the range within which the efficiency for given abscissa-value may be practically accounted constant. How far this range exceeds that covered by the model experiments (*see* § 20) we cannot confidently judge still less are we in a position to assign any definite value to the loss in efficiency that must accompany any given deviation of pitch-ratio beyond that range. But from indications afforded by the experiments, so far as they go, coupled with the reasoning of the I. N. A. Paper of 1878, already referred to, I am inclined to believe that although the range may extend to much larger pitch-ratios than the 2·2 value, it does not extend to much smaller pitch-ratio than the 1·225. I should therefore think it preferable to reduce diameter, if necessary, to an extent involving a large increase of abscissa-value (say up to = 15·0 even), rather at any rate than adopt a pitch-ratio lower than = unity.

DISCUSSION.

The PRESIDENT: I do not want to stand above a moment between you and the discussion which I hope will follow this Paper, but I have a duty to perform in this Chair, and I must do it, and that is to see that our Rules are strictly observed. I must point out to the meeting, but I do so with the greatest deference to all the intellect bestowed upon it, that that Paper has largely exceeded the time allowed for reading Papers by our Rules. Moreover, Mr. Froude has also appended to it a considerable portion of matter not included in his Paper at all. I wish, Gentlemen, to call attention to this fact—I feel it my duty to do so—in order that we may avoid any inconveniences that may arise from other transgressions of our Rules, and from matter of extreme difficulty being submitted to us which is not included in the printed Paper laid before our members, because unless everybody has a fair opportunity of seeing it and digesting it beforehand, it is perfectly impossible that they can join in the discussion with any advantage to themselves or to the audience. I felt it my duty to make this observation, and I hope in drawing up Papers, even of this abtruse character, our Rules will be strictly adhered to in the future. Half an hour is the time allowed by our Rules for the reading of a Paper, and this has taken nearly an hour.

Mr. Froude: I do not think I began till ten minutes past, my Lord.

The President: Still, that is more than three quarters of an hour. I am sorry to make these remarks, but I am sure you will feel with me, that, being in the Chair, I am bound to see that our Rules are not departed from.

Mr. Sydney Barnaby: My Lord and Gentlemen, Mr. Thornycroft desired me to express his regret at not being able to hear Mr. Froude's Paper. He just had an opportunity of reading it, and wished me to say how much he appreciated its value. I venture to make a few remarks because Mr. Thornycroft made numerous experiments at Chiswick a few years ago, which have been referred to by Mr. Froude, and I acted as his assistant, and am pretty familiar with his views upon the subject. I will therefore endeavour to say what he would have said much better. Mr. Froude has treated the results of his experiments in a very much fuller and more complete way than we have done, although both have approached the subject in a somewhat similar manner. I will try and point out as well as I can how far our results are corroborated by Mr. Froude and also where there appears to be a want of agreement. They agree very curiously, as I notice on Fig. 4, in the absolute value of the efficiency. It is a point of minor importance perhaps, but it is a fact that the $\frac{7}{10}$ths efficiency given there is exactly the same as that we obtained with our best propeller. But there is this important difference, that while Mr. Froude found, to his own surprise as he states, that within so large a range of pitch-ratio as from 1·2 to 2·2 there was little if any difference in maximum efficiency, Mr. Thornycroft's experiments seem to give a much clearer indication of the best ratio of pitch to diameter, for the best efficiency, and curiously this ratio was lower than the lowest included in Mr. Froude's experiments, viz. 1.15. The performance of this propeller is shown in Fig. 1 (Plate xxxii.), and marked curve of efficiency. Mr. Froude has shown how you can deal with a number of these curves of propellers having different pitch ratios, but finding as we did that our results differed so widely with propellers of different ratios of pitch to diameter, we selected the best and constructed from that the curve shown in Fig. 2 (Plate xxxii.) a curve of diameters and revolutions for different speeds and horse-powers, which does in effect what Mr. Froude has done with his own. If you compare our curve of efficiency with Mr. Froude's you will see that ours is a much more humpy curve. It maintains the maximum efficiency over a much less range of speed. It is very satisfactory to find from Mr. Froude's more extended investigations, that the limits of good work are probably not so restricted as appeared to us to be the case. It is our opinion, and I am sure Mr. Froude will agree, that as engines exist only to drive the screw, they should be subordinated to it, and that when possible the most suitable diameter and revolutions for the propeller should be first selected and then the size of the engine should be fixed. However, when you have to deal with given engines you must get a screw to suit them. We are not able to do this with our curve, but Mr. Froude is with his. He has pointed out what is very important, and what impressed itself upon us, viz. that you can easily have a screw too large. If you do that the efficiency is reduced very rapidly indeed, but you may have a screw smaller than that dictated by the curve and working at a higher slip without reducing the efficiency very much. This has the advantage, as he pointed out, of enabling you to run your engines faster, and so get them lighter, without affecting in any considerable degree the efficiency of the screw. The diagram (Fig. 2), though not covering nearly so much ground as Mr. Froude's,

has still perhaps the advantage of simplicity. You see the abscissæ scale is laid down in knots per hour. On the right the scale of ordinates is in inches and shows the diameter for one horse-power. On the left you have the revolutions per minute. For any speed you like (say 20 knots) you can read off the diameter for one horse-power. It is 2·6 inches. If you multiply that diameter by the square root of the indicated horse-power which the engine will develop you get the proper diameter for that speed and horse-power. Whatever the speed and horse-power the method is the same. The pitch ratio is constant. If the revolution given on the other curve for the desired speed be divided by the square root of the one horse-power the proper revolutions for the propeller will be obtained. One difficulty which we have found is how to pass from what Mr. Froude happily calls the "phantom ship" to the real ship. What can be determined to a nicety with Mr. Thornycroft's curve is the best diameter, pitch, and revolutions for a screw to work behind a phantom ship, when it is known what the power transmitted through the screw shaft is, but the effect of the following current is most difficult to estimate, and here I have failed to follow how Mr. Froude meets the difficulty, although I observe it is met, and no doubt successfully. A method we have found to work out well in practice is this. By increasing the disc area in the proportion $\frac{\text{I.H.P.}}{\text{Brake H.P.}}$ we find that the slower speed with which the screw passes through the water behind the real ship is compensated for; of course this makes no allowance for difference in the form of ships and the efficiency of engines. It suits very fine vessels. Full ships would require a screw of large diameter and less revolutions to work at the same efficiency. I may say we have verified this lately. We made numerous trials with full-size screws upon a torpedo boat, and by varying the diameter and the pitch-ratio we obtained what appeared to be the best. Although the I.H.P. varied with every reduction of the screw and the speed also varied with each alteration, we found that the diameter obtained by experiment as the best agreed within ½ inch with the diameter given by the curve for the particular I.H.P. and speed obtained with it. I should like to ask Mr. Froude whether his experiments agree with ours in one particular. He referred to it, but I was not quite able to catch it. It is the effect of broad blades. I would say that what we found was this—that whether a propeller has narrow or broad blades the same thrust is obtained with a given diameter at maximum efficiency. I think that is all I have to say, my Lord.

Mr. W. H. White: Mr. Barnaby will be ready to publish these diagrams in connection with his remarks, in the *Transactions*.

Mr. W. Denny: My Lord, I cannot say that I have risen to speak on this Paper as one who has followed Mr. Froude's work with the same care as the first speaker, for although my firm has an experimental tank, we have not yet a propeller truck, nor have we yet begun such investigations as Mr. Froude has been carrying out with so much ability and energy. But while unable to enter at all minutely into the merits of the Paper, I can speak of some of the effects of the work carried on by Mr. Froude and his father before him. One reason I have for speaking is, to thank Mr. Froude on behalf of my firm for the great generosity and kindness with which he aided us throughout in building our tank, in producing the apparatus we are now using in it, and the screw-propeller apparatus we hope soon to employ. I think an indebtedness of this kind ought to be publicly and fully acknowledged.

Mr. Froude: Mr. Denny would allow me to say it is the Admiralty who are to be thanked.

Mr. Denny: The Admiralty also have our best thanks. It is however said "You may bring a horse to the water, but you cannot make him drink," and although the Admiralty, through Sir Nathaniel Barnaby very kindly directed Mr. Froude to give us the necessary information, such information might have been given grudgingly, instead of generously. It was given most generously. Although we cannot follow all the reasoning in this paper, or immediately grasp all the practical results flowing from it, such practical results as Mr. Thornycroft has also been obtaining, we can yet highly value such work. In the interesting paper which Mr. White read to the Institution on Wednesday, he mentioned the range of efficiency of the later twin-screw vessels of the Royal Navy. I am able to form some opinion upon this range of efficiency, partly from information kindly given me by Mr. Froude, and partly from work which we have done in our own tank. We have been investigating very carefully, and especially for merchant vessels, the comparison between the efficiency of single screws and the efficiency of twin screws, and I may say that the results we have obtained with mercantile twin screws show that the Admiralty have by their perseverance, and under the excellent guidance of Mr. Froude and his late father, managed to raise the efficiency of twin screws very considerably—their range of efficiency being higher than that we have found general in the performance of merchant steamers. This success I note with pleasure, as it is one obtained not by any fluke, but a by patient and careful investigation, and by the support which the Admiralty has given to the Torquay tank, now transferred to Gosport. The good work done at Torquay is not only evidenced in these results obtained by the Admiralty, but also in the valuable papers contributed by Mr. Froude and his father to this Institution. Year after year we have had the subjects of resistance and screw propulsion brought before us by them in the fullest way. The winter before last I had occasion, in the course of a controversy I had in the North, to make a survey of the exact position of indicated horse-power analyses at that date, and in this Mr. Froude gave me every assistance. I was very greatly surprised at the advance which had been made, but this paper shows that the advance then made is suprassed by the advance now being made. No doubt Mr. Froude has introduced hypotheses into the work he is doing, but he frankly tells us about them. Mr. Froude's position is that of a hardy pioneer prospecting the way to a clearer and better road than we have had in the past, and although we may be following his steps with some discouragement at our inability to tread exactly in them, we can yet follow him with the hope that we shall at last gain the road, and recognize its advantages. If many of us cannot join Mr. Barnaby in conveying to Mr. Froude the assurance that we understand all he has told us, we can at least convey to him what I think he will appreciate, our sympathy with the work he is doing, and our very great hopes for its future.

Mr. C. E. Stromeyer: My Lord and Gentlemen, I fear that I am unable to improve on Mr. Denny's praise of the Paper, and will, therefore, content myself with asking a few questions and making a few suggestions. I have always taken a great interest in this question, and by exerting myself I was able to see the Paper a day or two ago, but without the diagrams. My interest was increased after perusing it, though I must confess that I was disappointed in finding more mathematics than I had

expected and less details about the experiments than I had wished for. There is one point about which I wish to be quite clear: was the maximum speed of the propellers no higher than two knots?

Mr. Froude: 206 feet per minute.

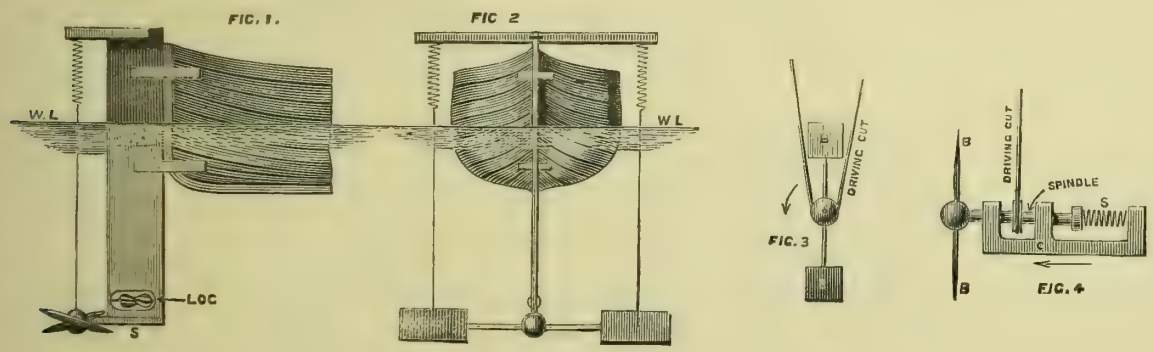
Mr. Stromeyer: That is as nearly as possible two knots, and it seems to me to be a very low speed from which to draw weighty and elaborate conclusions about speeds ten times higher and horse-powers probably 1,000 times greater. I was consequently agreeably surprised to hear that the conclusions for higher speeds are corroborated by Mr. Barnaby's experiments. Nevertheless, I hope that experiments at higher speeds will be carried out. The experiments which I mention in my Paper on strain indicators are too incomplete to be of any scientific value except in so far as they show how the actual thrust of a propeller in large steamers can be measured, and I hope that Mr. Froude will be able to persuade the Admiralty to measure the thrust of their propellers on any future trial trips, especially in such cases where the models have been experimented upon by him. Another disappointment I experienced in reading through the Paper was the absence of any experiments on the normal pressure on planes passing through water at an oblique angle. Such experiments would constitute the elements of the theory of screw prepellers. I therefore sincerely hope that Mr. Froude will on a future occasion be able to favour us with such experiments. Mr. Beaufoy was the only man who had experimented on this subject, but that was in the days before screw propellers or even steamers were thought of; it is, therefore, not surprising that his experiments are very imperfect. His highest speed was only four and a half knots. He did not measure the frictional resistance of his planes when moving lengthways through the water; while the normal pressure on the planes, which is the quantity I wished to know, can only be found by dividing part of the resistance of his models by the sine of the angle of inclination of the blades, and is necessarily very unreliable. Nevertheless, a very careful analysis of these experiments has led me to the conclusion that the theories which give the resistance as the sine of the angle are quite incorrect; and with them, of course, any conclusions based thereon. On account of these various drawbacks which naturally reduce the values of these experiments, I shall not trouble this meeting with the formula,* which I have found expressed the normal pressures nearer than any other I could think of, but I may mention that I calculated the thrust of two twin propellers with the help of that formula, and found that it was equal to about fifty per cent. of the indicated horse-power with a slip of twenty-five per cent., and that under these conditions both thrusts would be equal though on account of vast differences in area and pitch of the two propellers this was not the case for any other speeds. Reverting to Mr. Beaufoy's experiments, I would suggest that experiments of the following nature should be carried out. The disturbing influence of the friction would be eliminated, and the inaccuracy due to conversion of resistance into normal pressure would not exist. To the stem of a steam launch a vertical plank would be

* Note.—The formula expressing the normal pressure in pounds on one square foot of blade is—

$$3y^2 + \frac{y^2 x}{4} + \frac{x^2 \sqrt{y}}{3}$$

where y is the normal and x the longitudinal component of the speed in knots per hour. The tangent of the angle of inclination is $y \div x$.

attached which would carry a crossbeam at its upper end, while its under water end would have a spindle attached with a boss to which two blades would be fastened by conveniently long arms, and so arranged that they can be set at any angle. These blades would be held in a horizontal position by two wires secured to the crossbeam by two spring balances. When driving the vessel through the water the normal pressure on the oblique planes could be measured for the various speeds as recorded by the log (see Fig. 1) which would have to be arranged to record the speed on a dial fixed above the water. The depth of the instrument below water and the angle of the blades can easily be altered. Figs. 1 and 2 show respectively front and side elevations of this arrangement. For speeds of from twenty to sixty knots, which are those at which the tips of propeller blades travel, the following arrangement (Figs. 3 and 4) would be better adapted: Two blades B B, are immovably attached at right angles to



a spindle, which is free to move in a longitudinal direction in the carriage C. A spring balance presses against the after end of the spindle, and is set to revolve by the driving gut and small pulley while the carriage is moved in the direction of the dart. The thrust for various speeds can then be easily measured on the spring balance S. The angle at which the blades strike the water is found by dividing the forward speed of the carriage by the circumferential speed of the blades.

Professor J. H. Cotterill: My Lord, I do not propose to attempt a detailed discussion of this Paper, but as I have long taken a great interest in its subject, I should like to express my sense of the value of the work Mr. Froude is doing. It is only by systematic experiment that we can ever hope to understand a question of such great complexity. In my opinion Mr. Froude is perfectly right in the course which he has pursued of dividing his investigation into two parts; a part relating to "hull efficiency," and a part relating to "screw efficiency." In his very interesting Paper read here three years ago, Mr. Froude confined himself to the "hull efficiency;" that is, to the effect of the hull in producing a current following the ship, and the effect of the screw in augmenting the resistance of the ship. The results he obtained are remarkable. First he found that the actual wake current, with its infinitely complex motions, might, for all practical purposes, be replaced by an equivalent uniform current. This is a very important conclusion. In theoretical speculations such an assumption has often been made, but it had never been proved to be legitimate. More than that, in a very ingenious

and simple way Mr. Froude finds quantitatively the speed of this equivalent current, and, as it seems to me, thus proves that the wake consists in great part of motions induced by frictions. This is also of primary importance, because the neglect of frictional motions is one principal reason why many of Rankin's investigations—for example, his augmented surface formula—have proved unfruitful. In the further instalment which Mr. Froude now gives us, he takes up that branch of the subject which he describes as "screw efficiency." In the title of the Paper it appears to me that the word *diameter* would be more correct than *dimensions*, for so far as I can see, no attempt is made to determine the best blade area, nor is it even stated what the actual blade area was of the model screws employed. Yet I apprehend that blade area is a great element in the question, and this may be the reason why, in these experiments, pitch-ratio had little or no influence, a result which is certainly surprising. I presume, then, that Mr. Froude has in contemplation a third branch of the investigation; namely, a study of the effect of varying the form and area of the blades. It appears to me that to make these investigations complete, a set of experiments is wanted on the effect of varying the blade area in screws of the same type, and varying the form of blade in screws of the same blade area. I do not understand, from the Paper, whether Mr. Froude is of opinion that, within reasonable limits, blade area and form have no important effect on the efficiency. My Lord, I will now conclude by thanking Mr. Froude for the results which he has given us. I only hope that we may have more Papers of this kind from him, I am sure they are much appreciated.

Captain J. D. Curtis, R.N.: My Lord, there is one question I should like to ask Mr. Froude, and that is, What is the difference between the exerted power on the screw by the engines and its efficiency? I understood his father to say there was 45 per cent. loss of the working power exerted on the screw and the result on the vessel in her going ahead. I should like to know whether that is not due to the screw being so close to the ship? The late Robert Griffiths said by experiments made by him he found it should be two-thirds of its diameter clear of the stern post; then it would be out of the influence of the following water: otherwise it acted in a return retropulsive manner on the water following the vessel, and retarded the speed.

Mr. W. H. White: My Lord, I want to confirm what Professor Cotterill has said: in my judgment, and probably in the judgment of those who have had to do with trying screws beyond the conditions of previous experience, one very important feature has been found in practice to be variation of blade area and in form of blade area. This Paper, I think, will require to be supplemented by the further series of experiments that Professor Cotterill has referred to, and I do not think that Mr. Froude will take any exception to that view of the matter. He does, in the Paper, refer to the influence of numbers of blades upon performance, and to the question of width of blade, but in his judgment no probable variation in width of blade as governing blade area would exert a large influence upon the efficient performance. That I take to be his view; in fact, I know it is his view, because I have conversed with him on this subject. I can only say that in some of the trials with which I have had to do, in ships running at very high speed with engines running very fast, we found the contrary to be true. It has been found that variation not merely in diameter and in pitch ratio, but in the area

of blade, and in the disposition of that area in relation to the axis of the propeller, has had a very marked effect. Of course I am aware that in steam trials of ships other things may come in besides those which you think are being tried. That is a distinction which is necessary to be kept in view. Therefore I say I think this matter can be best settled by Mr. Froude undertaking that further investigation to which Professor Cotterill has invited him, and trying whether, in the different variations within the limits of probability in the form of blade, he gets any variation in performance. There is no doubt I was not far from the truth in my Paper on Wednesday, when I said that some of the results I then gave showed how little we knew about screw propellers and their performances; and if Mr. Froude, attacking this very thorny subject as he does, and getting on step by step, is able only to fix for us the best diameter; or if Mr. Thornycroft's investigations, in which Mr. Sydney Barnaby takes so important a part, help us to fix the diameter, we get one step forward, and are so much the better off. I have in my hand the result of experiments made not long ago, which show that there is a very real and great economy in spending a little money on such changes. There is the case of a ship which has been in service in the Navy for some years, and in which everything remained unaltered except the propeller, the diameter of which was reduced from about 20 to 18 feet, and the blade area reduced about 23 per cent., the pitch remaining nearly as before. Instead of developing 1760 horse power, 2260 horse power was developed from the same engines and boilers, and the speed went up from 12 knots to 13¼. These are facts, and there are plenty more such facts no doubt concealed. Mr. Wright wishes me to state that this ship was brought into the service, and had on her a mercantile screw, so that it is a case of general interest. Gentlemen who are of more authority than I can profess to be as to the practice in the mercantile marine, assure me that the biggest screw that can be put in on the working draught of water is usually looked on as the best, so that this has a very important bearing on the economical performances of a great many ships. I merely give this as another instance confirming that which we have had so many examples of, the wonderful economies which may be hidden away in possible changes in the screw propellers even of existing ships, leaving out of account what we may arrive at in the future, and I am sure that Mr. Froude will help us in this matter in a way that nobody else can do.

Mr. A. E. Seaton: My Lord, there is one point in the experiments both of Mr. Froude and Mr. Barnaby that I think is worthy of cognizance, especially by those who, like myself, have oftener to do with cargo steamers than very fast ships or war vessels, that all these experiments and results of experiments given in this Institution, are from ships with exceedingly fine lines, and driven at high speed or designed for high speed. I think it would be highly instructive if Mr. Froude or Mr. Denny would give us some information; probably, Mr. Denny could favour us with it, relative to the best proportions for screw propellers with bluff-lined ships. I think the ships that are dealt with both in Mr. Froude's Paper and in Mr. Barnaby's remarks, probably have a co-efficient of displacement not exceeding ·45 or ·46. Now we often have to deal with ships with a co-efficient of displacement something like ·75, and sometimes even higher than that, and we find that rules that are deduced from experiments such as these do not apply in the least degree to those very full ships. I have myself attempted, from the information so generously given by shipbuilders and others here, to lay down

rules whereby we can decide beforehand the best propeller for a particular ship, but without satisfactory results because the information is got from very fine lined ships only and moving under very different circumstances from the full shaped cargo boats, we find with these cargo boats that we are bound to have a screw of large diameter. We may settle the surface and the pitch to suit our own ideas or for experimental purposes, but to get anything like good results especially with a ship whose stern lines are full, we are bound to have the screw of large diameter. Then again, I must remind you that all your experiments are made in smooth water. I, unfortunately, have made one or two mistakes by fitting a screw to a ship which did excellently well on the trial trip but badly at sea. It is only very recently that I have altered the screws of some fast little boats from three blades to four and got no better result in smooth water, if anything, a little worse—the difference however was very slight ; but they tell me that at sea the improvement is very great indeed. So I think there may be causes which do not enter into Mr. Froude's calculations that we have to deal with, and this is one : we have to deal with ships that have to make a good average voyage, not a good result on the measured mile only, and we find the four-bladed propeller will do that better than the three. I have tried many experiments as to the relative surface and pitch. I have not put the results together in such a form as to speak of them to-day, but we got some very remarkable ones at times. One gentleman asked a question as to whether the thrust varied much with a propeller of the same diameter but with a different surface. My own experience is, that it does not differ much when the ship is running free, but if we have to build a boat such as a tug or one that has to do work somewhat analogous to towing, we want a very different kind of propeller to what we should require if a ship was simply running free. It is no secret – everybody knows it—that a tug must have a propeller of large surface. Then again the shape of the blade has something to do with it. There is a gentleman who has a patent. Mr. Ross, I think, bought it out. I tried Mr. Ross' plan a long time before ever I heard of Mr. Ross, and in some particular ships got a good result. It is an attempt to concentrate the flow from the propeller. We got a good result with one ship from that—a ship driven at about 14½ knots, but we could not please the captain. He said he did not like the patent. He admitted it drove the ship at a good speed, but he said it did not convey to the ships he was passing the idea that he was going fast because it did not kick up a bobbery behind. I noticed, in a trial trip the year before last with a fast twin-screw boat, a very remarkable action of the screw—at least, remarkable to me. At times the screw seemed, "to take charge of the water." It seemed to get an abnormal slip, and caused a small wave to rise over it, as if the water were being whirled without effecting propulsion. We noticed that when the ship got to 13 knots the propellers did well, but between 13 and 14½ knots this taking charge of the water occurred at fixed periods, and consequently we got comparatively poor results from that ship. We afterwards substituted a propeller of the same diameter—the same number of blades but with a slight increase of the surface, not very much in quantity but the surface disposed more towards the tips and we got in that case a very much better result. The indicated thrust up to the point at which efficiency had apparently decreased with the original propeller, was the same as with the other propeller but beyond that the propeller with broad blades at the tip did much better. There was one remarkable result in that particular ship. After the trial trips and after the ship had been taken over and run for

six months, she came in for survey, the superintending engineer rather disagreed with the number of revolutions: which was at full speed 89 or 90, and wished us to alter the pitch of the blades to bring the revolutions to about 80. We brought them down to 82 and the ship was put on her station again, now, whereas previous to the alteration, there was no difficulty whatever in keeping steam, the engineers found that there was a considerable difficulty in keeping steam. That is not the first time I have found the same result, but it was more marked in this case than before, because this ship had large boilers. I should add that the valve gearing in this case was never touched. There is another point (perhaps I am going back to the other Paper), but I see no special mention made of the effect of driving these little twin-screws ships, in shallow water. Many years ago there was rather a remarkable result from a twin-screw ship running between Hull and York. She was fitted with twin-screws interlocking, in which the ratio of pitch to diameter was, I think, about three. It was I know very extreme. The propelling effect was very bad indeed, but the most marked thing that happened about it was, that the boiler would not supply steam enough and when the ship got into shallow water she drew considerably more water than when she was in deep water; in other words, she sat down aft and dragged in the mud. Other screws were substituted for these with very much finer pitch; the revolutions were increased from 100 to a little over 150; the valve gearing was not touched; but the speed of the ship was increased by at least 50 per cent., steam was easily kept up, and the tendency to suck or sit down quite disappeared. I will conclude by again expressing a wish that Mr. Froude or others will enlighten us, who have the misfortune to dwell on the East Coast of England, with a little information, to guide us in accounting for some of the peculiarities of the bluff cargo boat; it would be of very great value, because the screw of the bluff cargo boat at the present time is scarcely satisfactory to us.

Mr. R. E. Froude: My Lord, at this hour I will not detain the meeting by making many remarks, but I must thank the gentlemen who have spoken, for the full way in which they have discussed the Paper, and must thank both the meeting and yourself, my Lord, for the patience with which you heard me out, and the kindness with which you passed over the infraction of the rules of which I was hardly conscious. In reference to that, I am not clear whether your lordship was referring to the paragraphs I read from the manuscript at the end of the Paper, or my interpolations. With reference to the former, perhaps I might explain that I sent the paragraphs in question to the Secretary immediately after I sent my revise of the Paper, and I had hoped they would have been able to be printed with the Paper. I was not aware that by sending it at that late date I was infringing any rule. But if, my Lord, you refer to the remarks which I interpolated in reading the Paper, I may perhaps be allowed to say that, so far as my recollection goes, I did not say anything that was not in the Paper; but, on the contrary, omitted a great deal that was there, and I think that Mr. Seaton has reason to complain of that omission, judging from those of his remarks which imply that results of this kind do not take account of the variations in the forms of ships. The Paper explains how, in using these results, account may be taken of the difference in fineness of ships, and the effect of such difference upon the difference between thrust and resistance, and between what I

have termed the speed V_1 and the speed V—. If Mr. Seaton will work out the dimensions of screws suitable for various ships, according to the diagrams accompanying this Paper, he will find that an increase in fulness of form will give precisely the result he has found in practice; namely, the necessity of an increase in diameter of screws. It would be quite impossible to allude to the remarks of all the speakers, or to all the points which have been referred to as not touched upon in the Paper. All I can say is, that if all these points were treated effectively in a Paper of this kind, not one, or two, or three Papers of the regulation length would suffice for the purpose. The points which have been referred to, to which I must confine my remarks, are the questions of the width of the blade and the form of the blade. Perhaps I might explain that the model screws on which these experiments were made had blades shaped thus [*illustrating on the black board*]. I do not remember at this moment the precise ratio of width of blade to the radius. This represents the developed outline of the blade, and the rule which I have gone upon in making the blades is to make the developed shape of the blade an ellipse, of which the major axis is the radius of the screw. I do not pretend that there is any special virtue in that exact form, but I believe it is somewhere about the best, and I have adopted the precise form of an ellipse because it is a convenient way for a variety of screws similar in character though differently proportioned. In reference to the question of the best proportion of width to radius of blade, I must admit that I have very little experimental information, therefore I cannot say how far I agree with Mr. Barnaby as to the proposition which he mentioned, namely, that the thrust, for maximum efficiency was not affected by variation of width of blade; but I may point out that this proposition, which is given, Mr. Barnaby states, may be deduced from his experiments, is in favour of the assumption which I have used here, namely, that the differences in blade area may practically be left out of account. I quite admit this question of the effect of difference of blade area is one which it is desirable to pursue, and no doubt there is room for a good deal more information with regard to it. I quite think that the form into which I have reduced these results is perhaps, in the elaboration of its framework, so to speak, worthy of rather more actual definite information than has yet been put into it. One more point I may refer to has reference to the efficiency curve given by Mr. Barnaby's experiments. He points out that it does not quite agree in character with that given by these experiments, being more sharply curved near the point of maximum efficiency, and dropping faster with the increase of the abscissa value. I think that impression is derived partly from the difference of scale on which the diagrams are plotted. The abscissa scale to which the efficiency curve is plotted in the diagrams of my Paper is very much more extended than that on which Mr. Barnaby's efficiency curve is represented, and I think that if the two curves were represented on an equivalent abscissa scale, you would not see so very much difference. I think that is all I need say, except to again thank the meeting for the indulgent reception which has been accorded to my Paper.

The President: Gentlemen, I am sure you would wish me to convey your united thanks, to which I cordially join my own, to Mr. Froude for the value of his Paper, and I am quite sure he must be gratified and encouraged by the high encomiums passed upon the Paper and upon his previous researches by some of the highest authorities in this room. I only wish to express to you, Gentlemen,

(and this is a personal matter), and to Mr. Froude, the extreme pain I may almost say, with which I felt compelled, in discharging the duties of the chair, to call attention to the time consumed in reading the Paper. That really was a matter of duty which I felt compelled to do, because I must remind you, Gentlemen, that my duty is not only to study the interests of individual members, however eminent, but also to study the interests and time of all members, so far as it is in my power to do so, and if I allowed latitude in one case, in common justice I should be bound to do it in another, and then probably we should have to sit here for a fortnight, instead of a week, to get through our business. That is the whole secret of the thing, and I am sure, if I have wounded Mr. Froude in any sense, that you and he will equally forgive me for doing what I believe to be my duty.

DESCRIPTION OF AN INSTRUMENT INTENDED TO ANALYSE ROLLING.

By Vice-Admiral E. Paris, Honorary Associate.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

It is not necessary to have been to sea in order to know that there is no repose on rough water; one must, however, have actually experienced rough weather in order to be able to imagine what are the movements produced by the waves, with what ease they move the largest ship as well as the smallest, and even cause the most injury to the heaviest. There is no physical movement which shows the rapid motions of such heavy masses. Neither the wind nor machines could ever move, one might say, with equal ease, the old three-deckers of 4,500 tons, or the armour clads of 12 to 15,000 tons, without speaking of the "Great Eastern," which rolled immoderately in spite of its length of 210 metres and its width of 25 metres.

It is, then, not surprising that this question has attracted the attention of men of science, and several have treated it in a highly analytical manner; amongst others and recently M. Bertin, who is now the Director-General of Naval Construction in Japan. He published on this subject a book entitled, "Waves and Rolling," published in 1874, by Mons. Gautier Villars. On the other hand M. Antoine, Ingénieur de la Marine, published in 1876 two scientific memoirs, containing numerous valuable documents, amongst others a method of calculating *a priori* the number of oscillations per minute of vessels of different types. It may be asked how I, who have been at sea constantly from the age of 16 years, venture to treat such questions. I have only been able to acquire practical knowledge; but it is just this practical experience which has enabled me to see and observe many things, and it is on this alone that I rely in endeavouring to make clear, by means of an instrument, and a little reasoning, how it seemed to me that vessels rolled when at sea. I do not know if any anterior work has treated the same subject in an analogous manner, and it is consequently with much diffidence that I enter upon the subject. The movements which floating bodies undergo

have probably never been solved in a practical manner, seeing that the forms of ships have undergone very marked changes, while no one has been able to decide which are the most preferable. Their sides were first made inclined and tumble-home, then flat and vertical; these latter, after having been retained for several ironclads, have recently been abandoned in favour of the first form.

We have, consequently, here conditions so difficult to seize and to reduce to curves, or to figures, that the behaviour of vessels at sea appears to have been the sole guide to practise. Sea-going ships were called round vessels, not only because of their short and obtuse form, but also because their rounded sides receded above water. I have found these lines on very good authority in the vessel "La Couronne," of 1660, which was very much admired in its time, and the exaggerated features of which, mitigated to a certain extent, were preserved to about 1820. Thus the vessels of most repute of the last century were one-third narrower at their upper parts than at the water-line, and the ratios of $\frac{1}{4}$ to $\frac{1}{5}$ were pretty general. But if the exterior form plays no active part in calm weather, it is very much otherwise if the waves periodically immerse the upper parts. Practical reasons have caused the tumble-home sides to be abandoned; one of these, especially, was the want of room for manœuvering. I assisted at this transformation, and I saw that if the new vessels were equal to the old in fine weather, it was very much otherwise when they were tossed about by rough seas. The old type was certainly given to deep rolling, but it was gentle, while the new ones underwent very brusque movements, which produced considerable shocks, in spite of the amplitude of the rolls. I have been on a cut down ship which, relieved of the weight of its upper part and of its upper battery, had sides flaring out above the water; it experienced such violent rolls that it was necessary to adopt every precaution to prevent it from losing its masts. The forms, even above water, have consequently great influence, and, in order to give an idea of their variety, I have drawn (*see* Plate XXXIII.) several sections at the midship frame, given to me at Calcutta in 1836 by Mr. William Seppings. The exaggeration of the frigate, the "Vernon," constructed on the principle of Captain Symonds, may be remarked. The form of the "Druid" approaches that of the vessels of M. Sané. I could have added sections of more modern ships, but it would only have caused confusion. In any case it is evident that the rising waves produce very different effects on each of the types represented.

I have very lately had occasion to appreciate the action of the sea on the exterior of a vessel, for one does not see all that happens to one's own ship. But I experienced rough weather with three fine vessels of the type "Napoleon," and it was

then that I clearly saw that it was the above water shape which enabled the waves to act on the vessel, and that on it alone depended the enormous changes of immersion and emersion which have been observed by everybody, and to which their proper importance, it appears to me, has not been ascribed. On the one hand, there are hundreds of tons in the air; and, on the other, hundreds of cubic metres immersed. At certain moments the sea rises to the highest point of the ship, while at others the green surface of the hull becomes so extensive, that one might almost expect the keel to show itself, and it has even been published that this has occurred. Why are these exaggerated angles produced on the surface of waves which have only an inclination of 8 or 10 degrees? That seems to me to arise from two causes, one of which may be called the elasticity of buoyancy, that is to say, the effect of reciprocal immersion upon a slender body, held straight by means of ballast, and which, when plunged too deeply into the water to spring out again, falls back, and so on, in a species of pendulary movement, the elasticity of which, both in extent and duration, considerably surpasses that of springs, and would maintain itself indefinitely if an escapement could be made to return to it the quantity of movement lost. At sea, the crests and hollows of the waves play the part of this escapement. The consequence is, that when a vessel commences to roll, it would go on doing so, if there were no obstacle, until it had used up its pendulary motion; but, meeting the rising wave, it plunges into it and rises out of it more than might be expected, by a law analogous to that of falling bodies, with a motion accelerated in each direction. For the sake of clearness, I think that one of these movements may be called action, and the other re-action, the latter being added to the former at each oscillation, in such a way that if there happened to be synchronism the motion would go on increasing, as Mr. Froude has shown it to do in his remarkable experiments. In calm water the oscillation is only continued by the re-actions, which become less at each roll, in consequence of the resistance of the flat parts of the hull, of the keel, and of the bilge keels, the influence of which is very marked. Being thus hindered in its movements, the ship is soon in stable equilibrium on water, which, being calm, produces no differences of level on supernatant bodies. It may here be observed that the re-actions may be usefully employed in order to estimate the effects of those to which new impulses are added, such as those due to an escapement, or to the undulations of the waves. The heavy and variously disposed weights of a ship keep up this species of movement for a long time, and they consequently really produce the re-action, their effects varying in proportion to the square of their distances from the centre of oscillation; thus those which are situated near this point exert but little influence, while those which are more distant, such for instance as the spars,

become sufficiently energetic to endanger the safety of the masts by their violent reactions which are much more dangerous than the wind.

Thus, in a ship there is the shape upon which the water acts, either keeping it in repose or imparting motion to it, according to what it does itself; and, on the other hand, there are the weights, which, no matter what their shapes, act only by their inertia. I think, then, it is certain that the shape gives the motion by the transmission of the movement of the waves, and the weights reacting, modify it according to their quantity and their position. Hence, the form is active and the weights are passive. The above has, no doubt, been said before in various manners, but it is useful to recall it, in order to show its influence upon what follows. Sir Edward Reed, in his recent and remarkable work, "On the Stability of Ships," shows some curious examples of the positions which homogeneous and weighted bodies take in the water. A light and flat disc follows the water, and appears glued to it. If a weight be fastened to it below, it resists and the water frequently covers it. If a second weight be placed above, they balance each other's influence on the motions of the disc. If the weights be placed on the edges of the discs, their inertia modifies its movements. A long homogeneous pole floats on its side, and turns indifferently in any direction. If, by means of a weight the pole be immersed to two-thirds of its length, it remains motionless, because the variations of the displacement, following the position of the wave, are not large enough, and do not last long enough to overcome the inertia of the weights at each movement. It is on this principle that the wave tracer was constructed. But, if this same weighted pole be immersed too much or too little, it takes a regular movement of immersion and emersion, which would last a very long time, were it not for the friction of the water and the resistance of the flat end. These movements by their amplitude are the measure of what I have called, for the want of a better special appellation, the elasticity of buoyancy. What I have seen makes me believe that, between these two causes, the following conversation may be imagined to take place. The weight says, "Leave me alone and I will not trouble you. See, here is calm weather, and I compel you take up a definite position of repose; but, if you disturb me, I will react on you, for such is the law of inertia, and I cannot escape from it." The shape replies: "Let the sea remain calm, and I will not disturb you; but since it carries me, I cannot help being lifted up on one side and let down on the other; hence results the movement of the whole and of the weight, which forcibly return what has been given to them." They say then: "If you wish us to remain quiet, try to obtain from those who have made us one, a shape as quiet as that of the sphere, if that be possible, and let the authors of our being place us (the weights) in such

positions where we may have the least chance of being moved. Let them above all things avoid making the waves, which move us all, keep time with the action of inertia, for in this case our motion would never be finished, and we should make a complete revolution such as Mr. Froude has demonstrated."

These reciprocal causes have therefore known actions, which it would be very interesting to be able to represent in a simple manner by means of a special instrument, for example. But they are so confused that we are reduced to forming hypotheses, or to accumulating isolated elements in a manner probably inextricable; the methods of action of the separate causes differing to such an extent according to their position. Thus, admitting that everything acts at the centre of gravity, we have the simple pendulum, the metacentre being probably its point of suspension. But as a ship is very far from being a lenticular disc we must consider the dispersed weights; those on the horizontal line passing through the centre of gravity are naturally in equilibrium on each side; if they are put in movement they in no way resemble a pendulum, but much rather a fly-press, the axis of which is horizontal; they act solely by their inertia, they feel nothing of the effects of weight. There are also the vertical weights which pass through the centre of oscillation, or the centre of gravity, and whose distance from this centre changes greatly the preceding conditions and really turns them into a metronome, to which the masting gives great power in spite of its relative lightness, because of the length of its radius of action, which acts in proportion to the square of its length. I think then, that by dividing the vertical section of a ship by two lines, like a St. Andrew's Cross, and placing the metacentre and the centre of gravity in it, the pendulum might be represented by the line M G (Fig. 1, Plate XXXVII), acting with the whole of the weight, but with a very short lever, the general action of which is to obey the law of gravity, to return forcibly to the position of equilibrium in the vertical line, and to do so with more or less energy according to the length of the lever and the sine of the angle of inclination. Its action depends only on gravity, and its small amplitude gives it so little motion that its inertia plays a quite secondary role, in spite of the greatness of the total weight. But the weights situated round about this point, and especially towards the extremity of the horizontal line B B play a very different part, inasmuch as they are always mutually in equilibrium. But to such an extent are they set in motion by the impulses of immersion that their action becomes that of a fly-press, the motion of which latter would be stopped after a first impulse if it did not receive a second one after having stamped, say a coin, and that because it only swings to one side. But if it be so arranged as to strike a second blow in the opposite direction each of them will produce a *reaction* added to the new impulse, and if there is isochronism there will be cause to fear lest the

whole machine be broken. Moreover, the duration of the *reaction* will cause the oscillations to be short and frequent; the inertia being overcome immediately by the violence of the shock and the impulse. But if instead of stamping a piece of metal, one could fit up the fly-press with a sort of piston plunger, like that of a feed pump, and replace the coins with considerable thicknesses of india rubber rings, the shock would take a certain time in being absorbed by crushing these rings, and the return would be accomplished slowly in consequence of their elasticity; in such a way that the action and re-action being no longer brusque, the inertia would be absorbed slowly and the periods would be so much the longer as the re-action was more elastic. Whenever synchronism was approached or attained, the amplitude and the violence of the oscillations would increase. If the axis of the fly-press were horizontal instead of being vertical we should have a representation of what really takes place in the case of the lateral weights of a ship, such as her sides, artillery and armour plates. These weights should follow the laws of the fly-press, and that, too, with a period, an amplitude and a quietness which depend not only on the moment of the weights but also on the extreme elasticity which an immersed body experiences in rising and falling when moved from its position of floating equilibrium. The times of these oscillations, as well as their amplitudes are influenced not only by the weights of the different horizontal segments, but also by those which are situated vertically above the keel, such as the ballast and the masts, which latter act in a totally different manner as we shall see further on. Thus, for example, a very flat ship will follow the waves if it be light; but if its edges be weighted, the inertia of these weights and the fly-press motion which results from them will increase the motion if it be in accord with the waves, and will cause water to be shipped if there is discord. The practical difficulty is to determine the centre of action of all these weights and its distance from the centre of oscillation, so as to ascertain the energy of its special action, and it is so much the greater the more the weights situated on the lines $a\ a$ and $c\ c$ approach the vertical and consequently follow other laws. Here we have a practical difficulty, perhaps much greater than that of determining *a priori* the centre of gravity of a ship.

But by dividing the vertical by two diagonal lines we have seen that there were two other segments, $a\,\mathrm{M}\,\mathrm{C}$ and $c^1\,\mathrm{M}\,a^1$, the weights of which oscillate nearly horizontally, and would act very much as a fly-press if they were equal and at the same distance from the centre. But, inasmuch as the ship is stable, the lower weights predominate greatly over the higher; consequently they act towards the bottom like a pendulum, and towards the top like an inverted pendulum, which latter modifies the oscillations of the lower one. This disposition of the weights quite resembles that of a metronome, the

oscillations of which are regulated by the respective distances of the weights from the centre of oscillation, and by their respective amounts.

My colleague at the Academy, M. Boussinesq, has been kind enough to communicate to me the following formula :—

$$L = \frac{P\,d^2 + P^1\,d^{12}}{P\,d - P^1\,d^1}$$

which is the expression of the law which regulates the metronome. The duration of the oscillation is modified but little by the separation of the weights.

Considering the lower segment, it is to be remarked that the weights in it are very considerable, but that they act at the end of short levers, the ballast and the engine being situated but little below the metacentre, while the boilers, and especially the coal bunkers, participate chiefly in the kind of action of the fly-press. Higher up the condition of things is reversed, the only weights are the decks and the masts, but the lever, at the end of which they act, is enormous compared with the other, from which it results that they ought to act as a sort of moderator, because of the difficulty of suddenly reversing their direction at each oscillation. Thus, it has been recognised hitherto, that vessels deprived of their masts roll immoderately, and we should want to know if the throwing of their artillery into the sea and the lowering of their centre of gravity was a useful measure or the reverse, as far as regards the extent and the violence of the rolling. I think that the metronome-like action should have great energy, and that it should contribute more than the fly-press action to regulate the movements of a ship, no matter what may be the changes in the passage of the waves. It has also been long remarked that vessels preserve this property although the waves may be very different, and that among vessels of all sorts, sizes, and shapes, each rolls in its own fashion, although the character of the sea may be the same with them all. The small ones suffer more in a moderate sea than the larger ones. An idea of this can be formed by the excess of difference of dimensions between a large ship and a coasting vessel, and even the small floating box which is used for fishing for cod in heavy seas. The play of the inertia of the weights counts for much, if not for everything in these differences. In France, the squall which struck the squadron of Admiral Hugon in the Mediterranean has been long remembered. The smallest vessels suffered least damage, while the three-decker, the "Ocean" rolled to such an extent that it was said that the lower yard arms touched the water, and the body of the ship suffered so much that thirteen beams of the lower battery were broken. One thing worthy of remark was that no vessel was dismasted, which shows

the perfection of modern compared with the older type of rigging. The "Ocean" went before the wind, and her excessive rolling was probably due to synchronism with the waves, since the vessels which stood up suffered much less.

To recapitulate, the following are, I think, the three ways in which inertia acts on board a vessel floating in a rough sea. The first is the action of the simple pendulum. Should it always be taken into account? I am inclined to think not, inasmuch as the centre of gravity acts at the end of a lever arm, so short that its effects should disappear, as least to a great extent, when compared with the two other sorts of action, the fly-press and the metronome, which act through much greater distances, and thus accumulate much more force on the weights of a ship, so that their position gives to them the most importance. I think then that the pendulum should almost disappear before the metronome, of which, as a matter of fact, it constitutes a part, and that the phenomena which we are discussing appear to be reduced to a combination of the fly-press and the metronome, which, in the general combination, are mixed up and weakened towards their lines of junction $a\,a^1$ and $c\,c^1$ (Fig. 1 Plate XXXVII.), so much so, that we could picture the action to ourselves—very vaguely, it is true—by colouring each segment with a tint, which should become lighter in proportion as it got further from the centre, and which should eventually be indistinguishable from the equally light tint of the edges of the neighbouring segment; thus showing the places where their different effects become mixed up. But it is not in this vague way that a correct understanding can be established, and still less calculations made. If we attempted anything of the sort we should fall, no doubt, into a labyrinth of uncertain figures, intended to show the positions of the weights and their influence, which varies with the square of their distance from the point of oscillation, the position of which latter is very uncertain, and probably varies even when oscillations take place in calm water. On the other hand, when making experiments on an existing ship, it is difficult to separate the elements of the question, and to assign to each the part which belongs to it. It would, perhaps, be possible to approximate to the truth by making experiments on rolling with different conditions of load, such as an empty hull, engines completely in place, boilers without water, boilers full, armament or armour plates in place—lastly, everything empty, or full, as will be explained later on.

In any case it is convenient, before going further, to give one of the principal reasons which leads to the conclusion that the purely pendularly movement exercises very little influence on the motion of the whole. This reason is the extreme slowness of the oscillations, compared to what they should be, considering the distance of the metacentre

from the centre of gravity. Fig. 3, Plate XXXVII., shows the length of pendulum corresponding to the length of roll in seconds, for several vessels. To the figures of this table may be added the following extracts from the numerous documents in the interesting memoir of M. Antoine, Ingénieur de la Marine (1871), giving the number of oscillations per minute, which have been transformed into duration in seconds in order to compare with the preceding.

	Seconds.			Seconds.		
Rochambeau	16·	to	13·2	Screw boats	5·26	„ 3·5
Onondaga				Screw troopships	5·47	„ 3·13
Magenta	7·6	„	7·17	Sailing frigate "Alceste"	4·8	
Gloire	6·12	„	6·0	"La Protectrice" ...	3·1	„ 3 0
Ironclad corvettes	6·04	„	5·17	Frigate "L'Tis"	3·33	
Paddle wheel steamers	6·0	„	3·17	Brig "L'Obligado"	4·28	
Paddle wheel despatch boats	6·0	„	2·6	Troopship "Cyclope"	4·6	

At the end of numerous documents, of which the above are only a short extract, M. Antoine concludes that rolling is a function of the beam of ships, pretty nearly, but at the same time that the stowage exercises a most important influence.

The preceding figures show also that light vessels experience only slight movements, whilst those loaded with armour plates on the sides have long periods which, however, do not anything like equal those of the very flat ironclads, and this leads to the belief that the fly-press movement plays a very important part, since sailing frigates, whose principal side weights are their guns, but which have very high masts, have also the shortest periods of all.

To return to the successive operations mentioned above, it is necessary to say, that since they have not been carried out, it appeared that there might be some interest in doing for this subject what has been already carried out, in order to explain many physical phenomena; that is to say, to attempt to make an instrument which should group, in a simple manner, the special elements which appear to play a principal part in the rolling of ships, in such a way as to appraise them both separately and together.

As is seen by Plate XXXV., which represents two views taken at right angles, it consists of two sorts of pendulum, one of which, M M^1, is a true metronome intended to represent the approximately regular motion of the sea, which it does by varying at will the duration of its oscillations according to the opposite positions of its two

lead discs. With this object in view, the rod has been divided at every fifth centimetre in such a manner as to cause it to oscillate in different positions.

Upper Weights. Constant Position.	Lower Weights. Distance from the Axis.	Number of simple oscillations per second.	Duration of the oscillations in seconds.
Metres. 1	Metres. ·82	20·2	3·
	·72	28·	2·11
	·62	32·	1·87
	·52	37·	1.62
	·42	44·	1·29
	·32	48·	1·25
	·22	52·	1·15
	·12	55·	1·08

Irregularities may be remarked in these numbers, due to the permanent length of the rod, and more especially to the impulses, which were very irregular compared to those of an escapement. The rod M M^1 is carried by two steel knife-edges, C, resting across a slit in a plate of the same metal fixed to the wooden support, S, which is held by screws to the plate A A^1, the latter carrying the whole. At its upper part this metronome carries a wooden cross piece, T T^1, the centre of which is so arranged as to be able to slide without changing its inclination, so as to be at will removed from, or brought near to the centre of oscillation. This cross piece is pierced with holes at every ten centimetres in such a manner, that set-screws may be fastened into them, for the purpose of attaching pieces of india-rubber, the use for which we shall see later on.

The second pendulum N N^1 has a simple iron rod pierced with holes like the first and supported like it by steel knife-edges on the same plate as M M^1; so that the two axes of oscillation are in a straight line on the support S, and the latter of these two pendulums is in advance of the first, as is seen from the cross-view, moreover they beat perfectly independently and without meeting each other. The lower one, N N^1, carries at the bottom a cross-piece t t^1 which also can slide in the direction of its length and can be fixed to any desired point by means of a pin. This cross-piece is also made of wood so as to save weight; it carries two grooved leaden weights which are arranged to slide from the middle to the ends of its two arms and can be fixed by a pin at distances ten centimetres apart.

The holes mentioned above are used for screwing in little hooks to hold the pieces of india-rubber before alluded to, and which are shown by the letters C C.[1] Moreover, the rod N N[1] has a weight p^1 above its knife-edges which also can assume different positions, and which by its combination with the weights of the lower cross-piece $p\ p$ forms a sort of metronome, especially so when it is raised. It can also be placed below the cross-piece and then the upper part of the long rod N N[1] acts as an upper counter-weight, representing, so to speak, the masts. This second piece is intended to represent the weights of vessels in positions approximating to those which they would occupy on board ship and which we have supposed to be grouped in the segments of (Fig 1., Plate XXXVII.).

Thus the knife-edge c represents the metacentre which is taken to be the centre of oscillation ; P[1] placed high up is the weight of the upper armament, P[1] N the masts with their accessories, $p\ p$ the lateral weights of the sides of a ship, including the guns and in certain cases the armour, the inertia of all of which, acting at the end of long levers and with effects proportional to the squares of their speeds, are certainly of the greatest influence on the rolling. Finally, L is the ballast. It is obvious, from these arrangements, that if numerical data could be obtained to determine, or even to appraise the centres of action of the weights, we could dispose them and increase or diminish them in a proportional manner on the instrument and obtain a representation more or less approaching the manner in which everything acts on a ship, when the movement impressed on all its parts brings the inertia of all into play. To the above must be added that the cross-piece $t\ t^1$ serves also to transmit the periodic movement of the other piece M M[1] to the whole, which represents the ship, so as to imitate the impulses from below acting upwards and the corresponding movements from above acting downwards, which the waves produce when moving vessels.

In order to produce these effects with an approach to truth, it would be necessary to attach to the lower part of M M[1] by means of a cross-piece two vessels filled with water into which would plunge half-way two pistons like those of a feed pump. These pistons, connected by rods to the extremities of the cross-piece of N N[1], would have re-acted by an inverse movement up and down the vessels, the water in the latter giving the elasticity of buoyancy to the pistons. But the trouble of moving these weights of water would have rendered such an arrangement very difficult to carry out and very awkward to use. Consequently it appeared preferable to endeavour to produce an equally elastic effect, but in an opposite direction, by causing the cross-piece $t\ t^1$ and its weights to be drawn up, instead of being pushed up, by means of several thin india-rubber strips $c\ c$ fastened at distances

of five centimetres apart. This is effected by passing the strips through the pins and under the hooks of the two wooden cross pieces, in such a manner as to embrace every length and to give consequently every tension wished for. In this manner the action of the metronome M M^1, which represents the movement of the sea, is transmitted to the second piece, arranged to represent the ship, by means of very elastic pieces of india-rubber, which replace the pistons and water vessels and impress the efforts of the waves acting from below upwards, by means of the tension of the strips on one side; while on the other side the india-rubber having become slack leaves the part to which it is connected free to descend like a falling body under the action of gravity. I regret very much having been disabled by illness for a whole month which has prevented me from presenting to the Institution the results of any consecutive studies

In order to complete the instrument in question it only remained to trace the reciprocal motions on paper. To effect this two horizontal slits were made in the plank A A^1; the upper one *d d*, the lower one D D. A roll of paper B the edge of which is seen on the second figure, is carried on an axis in such a way as to be capable of unrolling. The sheet of paper passes through the slit *d d*, descends in front over the face of the plank, passes to the other side of the latter and again descends, and is rolled up below by the action of clockwork B^1 in such a manner as to make the paper travel forward. To carry the pencils the rod M M^1 carries above the knife-edges a long sort of rectangular ring O O in which the double rod N N oscillates freely. This ring carries at its centre a small tube in which is a blue pencil *i*, pushed forward by a spring in order to press against the paper and make a mark on it. Below this pencil the double rod N N carries a second pencil *i* with red lead similarly placed, which traces the rolling; the difference between the colours, enables us to distinguish the lines when mixed up on the paper.

I think that it is possible with this arrangement, to represent and even to study very concisely the relations between the inertia of the weights and the action of the waves in various circumstances. For the metronome enables us to vary the periods of the wave; whilst, by their tension the pieces of india-rubber represent the elastic impulse on the ship. The different holes in the cross pieces enable us to vary the distance from the centre and consequently the length of the paths travelled. On the other hand the changes in position of all the weights, which are easily effected by means of the little iron set screws, permit us to vary the effect of their inertia according to the space traversed. The instrument would, I think, be adapted to represent the influence of the differences of period examined by Sir Edward Reed, and also by M. Antoine, due to the relative angles of the direction

of the ship and of the sea, which show that, commencing, say at the north, we have a period of 5·7 secs. to the north east, of 6·8 secs. to the east, of 11 secs. to the south east, and of 3 secs. to the south, that is to say, with the wind behind. It would be easy to produce analogous effects with the instrument and also to represent the inclination which the wind, acting on the sails, gives to the ship. For this purpose it would only be necessary to have a weight p heavier than the other, or to bring one of the two nearer to the centre.

Now is the time to speak of the elasticity of floatation, a word made use of to convey the expressive English term buoyancy. This would have been easy to study with the wave tracer, the errors of which my son endeavoured to correct by experiments in a heavy sea by raising the pole above its point of repose so as to let it plunge back, or, by plunging it down so that it might spring up again. The two effects were nearly equal and the oscillations clearly showed the degree of elasticity, by means of the amplitudes of the curves and the periods as traced on a piece of paper; but unfortunately I have not been able to find these papers. I was consequently reduced to making an experiment on a small scale by means of round sticks, 1·5 metres long, the diameters of which varied from 8 to 1 centimetre. These were loaded with lead, so as to be immersed to two-thirds of their height. They were then graduated every centimetre in various colours starting from the point of rest. In raising them for the purpose of letting them fall, or in plunging them in so as to let them rise up again, the extent of their oscillations could be noted and translated on to paper as shown in Fig. 4, Plate XXXVII., which reveals the nearly regular degradation of the amplitudes in terms of the times, since it was observed that they were always isochronous.

The second figure, on the contrary, shows how much motion remains after each oscillation; thus, starting from the first movement, reckoned as 100, we have 84 per cent. for the reaction, or 16 per cent. of loss. The next swing marks the same figure, which can only arise from an error of the reading, caused by the rapidity of the motion, but if we follow the figures to the end, we see that the decrease of the reactions is very regular. Thus we see that the reaction was 85 to 90 per cent., the loss at each reversal of direction being 15 to 10 per cent. It is these last figures which, although not very precise, are nevertheless the most interesting, and an experiment, on a large scale, would make a useful standard for comparing the elasticities of different ships by means of co-efficients, which would be useful in estimating the rolling; the more so as they show what an important role the reaction plays in the phenomenon of rolling, for, if it is evidently

less with the shape and the large rubbing surfaces of a ship, it should still be recognised that it is always considerable enough not to be neglected without causing notable errors, and that it deserves practical observations as exact as those on stability.

This could, I think, be carried out in a very methodical and useful manner by commencing the experiments on rolling as soon as the vessel is launched, and when it contains neither engine, nor shaft, nor screw. An empty hull, especially when made of iron, is very well adapted to the calculation of weights and their positions. After each operation the calculated centre of gravity should be verified by Borda's experiment. For the purpose of producing sufficiently great inclinations, to estimate the influence of exterior form, it is doubtful whether men running from one side to the other produce a sufficiently marked effect, no matter what their number may be; and moreover their action can hardly be expressed in figures. I think, then, that it would be preferable to place crossways a strong spar held up by ropes which had been previously passed through a shackle, on the pin of which would have been mounted a sheave, in order to facilitate its movement along the spar. To this ring heavy weights would be attached, which would not be allowed to rise out of the water, so that the surface of the latter might not be disturbed when they were let go, more particularly when it is desired to read the angles on scales marked on the hull. It would have to be determined whether it would be better to make the experiments with a lever and constant weights, or if it would be preferable that the angle of inclination should not vary, which might easily be arranged by causing the weight to slide to a suitable distance from the ship. All this appears very complicated, but is not the solution of the question worth the taking such trouble? Would not such operations, carefully carried out and compared, in this epoch of precision in everything, be of great utility and furnish to scientific men numerical data for the construction of the most scientific formulæ? For, of the latter, one may say, that they are like good compasses, they show the route to follow, but we must, as at sea, add to them the numbers furnished by the clock and the log in order to arrive at our destination.

The same experiments of stability and of movement, and the estimation of elasticity which results therefrom, would be repeated when the engines were on board; the centre of action of the latter could be determined from the drawings, and the data as to weights furnished from the manufacturers. If there were armour plating its effect could be tested before that of the engines, and its centre of action determined more exactly than in the case of the other weights. Finally, the boilers, their water, the coal, the artillery, the

turrets, if there are any, and the masting could be submitted to the same tests, and Borda's experiment would be useful to verify the result each time. I fear that all this will cause alarm, especially in the dockyards, where the necessities of the service naturally take precedence of every other consideration, but I think if similar experiments, or better ones than I can suggest here, are patiently carried out, that great benefits will result from them.

To conclude, it is proper to give here some short examples of the combined movements produced by the two parts of the instrument and to replace, by dotted lines, the curve traced by the metronome which plays the part of motor, and by full lines the movements of the other part of the instrument, representing the ship. (Figs. 1 & 2, Plate xxxvi.) The first of these drawings shows the rolling in calm water, that is to say, equal on both sides. The second, on the contrary, gives an idea of what the rolling becomes when a stiff breeze heels the vessel over so that it does not roll beyond, or even attain to, the vertical position. The curve in this case is simply obtained by causing one of the weights of the lower cross-piece to approach the centre more nearly than that of the other. There results, consequently, an inclination of this cross-piece which the elasticity of the india rubber pieces permits, just as the water yields to the impulse of the wind and allows the ship to submerge one side more than the other.

There remains still one question to examine; it relates to the centre of rotation of the ship, which I have often wrongly called the centre of oscillation. Where is it really situated? Is it at the metacentre, like the point of suspension of a pendulum? Is it at the centre of gravity? What role is played in this connexion by the friction of the hull and the resistance of the keel, as well as of the flat portions at the stem and stern? Should they not, by their resistance, lower this point? I have not, in anything that I have been able to read, been able to find a method for determining this important point. On its position depend the lengths of all the lines which unite it to the weights in motion, and these influence the result according to the square of their length. I made, in 1850, a very rude experiment with another object in view, but which relates to the matter in hand. I fastened musket balls to pieces of string of about half a metre long, and suspended them by means of nails at equal distances from the floor of the stokehold to a sufficient height on the main mast. The higher ones were swayed very violently and nearly horizontally from right to left, while the quieter ones approached more nearly to the motion of a pendulum, and were situated a little below the water line. This was the position of least movement, and probably the centre of rotation of the ship. Would it not be advisable to make more exact observations, for example, with rigid pendulums, the points of suspension of which could be attached to planks carried

from the bottom of the ship to just below the upper deck. A pencil and spring could be so arranged as to trace on paper the arc traversed by each pendulum, and the place where this amplitude was least would probably be the centre of rotation. Such an experiment would be neither costly nor difficult.

Lastly, could there not be made for this kind of observation similar instruments to those so admirably contrived by Mr. Marey, which have replaced the human hands and obviated all chances of error in feeling the human pulse.

In the above we have only studied the passive effects of the motion of ships, without having attempted to estimate its cause ; that is because the difficulties of observation become greater ; they are hard to measure, or even to estimate plausibly. It is the waves which move the ship, but how do they do it ? Why do they, which are so similar to one another, act so differently on ships of different forms and sizes ? M. Antoine, Ingénieur de la Marine, has collected many experiments in order to give expressions for their speed, their mass, and their momentum. My son observed on the "Dupleix" and the "Jean Bart" that a very large sea had a velocity of 17·2 metres per second, while the wind had a velocity of 28·8 metres, the difference being 11·1 metres per second. The period of the oscillation of these waves was 10·3 seconds, their length from crest to crest, 148 metres, their observed height 7·7 metres, and the ratio of height to length 0·05. A wave one hundred and twenty metres long was 4·1 metres high, the ratio being 0·034. The water in the waves neither advances nor recedes, it only rises and falls successively, without having the rapid movement of translation which undulation simulates. It only produces on the ship effects of immersion and emersion, the action of which is always vertical. But how then does it act so differently according to the form of the ship? To explain this it is only necessary to draw vertical sections of two very different ships; one of the last century and the other of modern construction, and to describe on these drawings a circle, the centre of which is the middle of the water line, and the radius the half beam. (*See* Plate xxxiv.) If this circle were a cylinder instead of a ship it would represent the form most indifferent to the waves, as is proved by barrel-shaped buoys employed to mark dangerous spots, and the ballast of which is a chain. The difference between their tranquillity and the movements of a rectangular box is proof of the foregoing. In comparing the two sections by means of the circle, it is to be noticed that the emerged portions differ but little, but that the above-water parts are radically unlike one another. The one slopes away rapidly within the circle above the water-line, while the other forms a tangent to the circle, and this difference is still more marked if we compare the mean sections, represented by a dotted line, with the circle

which surrounds them: for this latter figure shows what takes place on the average from end to end of the ship. The difference between the surfaces, whether situated within or without what one may call the neutral circle, expresses, when multiplied by the total length of the ship, the plus or minus volume, which gives a very considerable cubic capacity. We cannot endeavour to take account of the effect of the waves except on the assumption that they come straight across the ship, and then a rough calculation expresses the differences of immersion, if we deduct them from the numbers found for the oblique lines of, for example, 15 and 30 degrees which represent the inclinations of the waves relatively to the ship, but not relatively to the horizontal line of a dead calm. But it would take too long a time to work out the figures thus for all the sections; it is much easier to perform the calculations for the mean section, and then the following figures are found, which are the resultant of the surfaces deducted from the mean section and multiplied by the length of the ship, which is supposed to be 80 metres. The following is the result of a rough calculation, the object of which is only to show the greatness of the forces which come into place.

	Angle.	In the water.	In the air.	Total.
		Cubic metres.	Cubic metres.	Cubic metres.
Vessel with "tumble home" sides	15°	520	560	1080
	30	968	1080	2048
Vessel with straight sides	15	640	552	1192
	30	1350	1182	2132

According to the above the vessel with straight sides would have 500 tons more than the other influencing the rolling after the fashion of a fly-press, that is to say, acting in proportion to the square of all the distances between the weights and the centre of rotation. Hence we can see the great difference in the action, according as the tumble-home sides shorten these lengths, or as the vertical sides increase them.

The sections represented show that the vessel with straight sides has less support on the lee side, and also that it is less sent back in the opposite direction than the other, whence it experiences less duration, and less amplitude of rolling. Fig. 5, Plate xxxvii., showing the ordinates of the water line for the two types of ship, proves the same thing; the full

lines show the ship when it is upright ; the small dotted lines when it has an inclination of 15 degrees, and the long dots, when the inclination is 30 degrees.

The plans of the two types of ships have been very much shortened in order to give the ordinates sufficient length to allow the notable difference between the two forms to be appreciated. The ordinates of the vessel with tumble-home sides diminish as the inclination is increased, while in the other case they increase, and that only applies to the exterior portion when it is emerged, for the hull experiences but little change in either case. And as these differences represent so much cubic capacity they show the great distinction between the action of the exterior forms in each of the types represented.

Now that men of war have no longer sails nor numerous crews to manœuvre on the deck, the inconvenience of the tumble-home side no longer exists. On the other hand the lowering of the weights and consequently also of the centre of gravity has not the same inconvenient consequences as when the shape acts too violently in inclining the ships ; consequently we may, I think, approach more nearly to the form of the most tranquil of floating bodies, that is to say, the barrel with considerable ballast.

To sum up, the following is what I venture timidly to say, as an old sailor, taking my stand on what I have been able to observe at sea. The shape transmits the movements of the waves. The weights receive the impulses and modify them. They appear in that respect to obey the laws of the fly-press and the metronome rather than that of the pendulum.

Experiments on the motion, carried out while the weights were being successively put in, and finishing when the armament was complete, would be as useful from the point of view of rolling as those on statical stability, and in the hands of scientific men they would aid in the establishing of laws which would produce results to which mere practice could only attain with difficulty.

DISCUSSION.

Admiral Sir John Hay : I think the thanks of the meeting are due to Admiral Paris for the interesting Paper he has given us, as well as for the valuable instrument which he has produced. I served with Admiral Paris in the Black Sea, and I know his distinguished character as a Naval Officer, and an ally of this country. I am sure we all recognize in this Institution the great value it is to us

when distinguished officers like Admiral Paris, who is not only well known for his great gallantry, but also for his scientific attainments, are kind enough to come here and shew the universal sympathy that is felt throughout the world in interesting subjects, of which this Institution perhaps takes particular care. I think myself happy to have had the opportunity of being present to-day to have heard my gallant comrade of former days giving us the information he has so admirably placed before us.

The PRESIDENT: Gentlemen, I am sure you will allow me, from the chair, to convey your thanks to Admiral Paris for his Paper, and this remarkable indicator which he has exhibited to-day. I would remind you, Gentlemen, that not only are we indebted to Admiral Paris for the results of what I may call his marine antiquarian researches, because it is with the antiquity of ships that he is so particularly interested, but also I may remind you that he has been a most generous donor of very valuable presents to this Institution, and his liberality is only equalled by his kindness of heart, and his readiness at all times to advance, as far as he possibly can, by his researches, the objects and the purposes of this great Institution. I am sure you will allow me to convey to Admiral Paris our united thanks and gratitude.

PROPOSED STEAM LIFE BOAT, WITH SPECIAL REFERENCE TO ITS STABILITY.

By Ludwig Benjamin, Esq., Member; and J. M. H. Taylor, Esq., Associate.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The widespread prejudice against steam as motive power for life boats will, we trust, not be shared by the Members of this Institution, and it appears not necessary therefore to take up your time by discussing this preliminary question.

The Steam Life Boat which we have designed and of which we have the honour of submitting models and plans to you, consists of a double ended boat of very great stability, 56 ft. long over all, 16 ft. 6 ins. extreme beam, with a draught of 3 ft. plus the depth of keels when fully loaded and with passengers.* (Plate xxxviii.) She is propelled by 4 screws mounted on 2 parallel horizontal shafts; the screws are located at about half the vessel's length, in grooves which extend in longitudinal direction for the greater part of the boat. The sides of these grooves are parallel to each other and to the middle line of the vessel, their highest points are in the way of the propellers, where the section of the upper part is a circle of only little more diameter than that of the screws, while the top of each groove drops at the ends and midships. By these means we keep the screws clear from wreckage, ropes, etc., while at the same time we protect the open surfaces of the grooves under the propellers by strong gratings. The proximity of the propellers to the midship section will allow an excessive amount of pitching before the screws are thrown out of action, while even then, racing is prevented by having mounted one forward and one after screw on the same shaft. The inside of the boat contains besides buoyancy chambers, three watertight compartments of which the midship one is used as machinery space, the two others as cabins, which hold combined about 70 passengers. The machinery consists of two pairs of compound surface condensing engines of 8 ins. and 16 ins. diameter, by 8 ins. stroke, arranged as low as possible at the sides of the shafts, which enter the engine room through the drop of the grooves by means of stuffing boxes. The boiler is situated between the engines and is

* These dimensions we have settled upon as best suited for general coast service; for special purposes they may be altered to suit circumstances.

worked with 120 lbs. pressure, the draught being forced by a fan. The machinery is capable of developing sufficient power to drive the boat at a speed of 12 knots per hour. In the machinery compartment space is set apart for coal bunkers, giving a supply for between 24 and 36 hours.

The boat is fully decked in a peculiar manner. At 6 ft. above top of keel, leaving 3 ft. freeboard when fully loaded, there is a deck of only such breadth as to allow a good passage, which widens into clear space at the ends of the vessel. The remaining part is taken up by a dome-shaped deckhouse, the top of which at the highest point is 10 ft. above the water. The entrances to cabins and engine room and all other openings to the boat are situated on the top of this house, and steps lead over the round sides of the same to the entrances. There is no sheer in the narrow deck round this house, but a slight drop at the ends, thus giving these the character of a turtleback. The house as well as the deck are of such form as to clear quickly of any water thrown upon them, while the shape of the boat proper is such as to lift easily through the waves. The inside of the house is taken up by buoyancy chambers, passing through which watertight trunks are provided for cabin entrances, ventilators, funnels, &c. The bottom of these buoyancy chambers are connected to the sides at such height as to allow sidelights for cabins and engine room to be provided above the narrow deck.

The place for the captain is a well in the top of the deckhouse with bottom openings through the buoyancy chamber, for clearance of water, and with a small house erected over it, which contains also the navigation lamps.

Two funnels from the boiler pass through the buoyancy chamber at the sides of the well, and unite above the wheel-house into one.

Stability.

As a life boat has to encounter forces of immense magnitude tending to capsize her, she must possess an extraordinary amount of stability. Life boats have been designed which possess self-righting power, but only very little initial stability, but such boats are unpractical, because although they always tend to come back to their keel, slight forces are sufficient to turn them through a complete circle of 360°. Others, as for instance the double tubular life boats, have a great amount of stiffness, but are not self-righting, and when once capsized they will remain upside down, as has been the case with the Liverpool Life Boat ; such boats are therefore equally unpractical especially if used as steam life boats.

We have avoided both extremes, and have given the lower boat an unusual amount of

stability, which under ordinary circumstances makes it impossible for hurricane and waves combined to heel her to extreme angles. By means of the shape of the deckhouse we have continued the stability in such a fashion as to make her self-righting, *i.e.*, the boat possesses only one position of stable equilibrium (the upright position) and she is bound to find this position speedily to whatever inclination she may have been brought by statical or dynamical forces.

The diagrams on Plate xxxix. explain these qualities fully, and the models, which are weighted and balanced according to the actual boat, prove beyond doubt the correctness of the diagrams.

As to the height of C.G., we have assumed the worst case, viz., the boat carrying 70 passengers inside the cabins and the coal bunkers half full, giving a total displacement of 36 tons. Under these circumstances, while the vessel heels to 180°, the loose weights will fall to what forms in upright position the top of cabins, etc. As the space of the deckhouse is filled by buoyancy chambers, this fall is limited by the bottom of these, and the C.G. shifts, from 4 ft. $1\frac{1}{2}$ in. in upright position, to 4 ft. $6\frac{1}{2}$ ins. at the inclination of 180°. This shifting height of C.G. forms the base of the calculations and is indicated in figures.

Fig. 1 (Plate xxxix.) illustrates the stability of our boat by the common diagram. It will be seen that the righting arm is rapidly increasing to the inclination where the deck immersion begins; it attains a maximum of 2·5 ft., after which a decrease takes place till the deckhouse begins to immerse, from whence the arm keeps pretty steady till the round part of the deckhouse reaches the water; a minimum is attained at about 100° inclination, of 0·63 ft., after which it rises again to about 150° to a maximum of 1·4 ft., and falls to 180°.

While the righting lever is therefore positive at all inclinations, the amount of dynamical stability is continually increasing till the vessel is heeled over to 180°, as shown by the curve in the same figure. This enormous amount of dynamical stability is considerably increased by the resistance of the grooves, keels and fenders, which have not been taken into account in the calculation.

Fig. 2 (Plate xxxix.) shows the heights of the metacentre above the C.G. for the different inclinations, by the diagram which was first brought before this Institution by Mr. Benjamin in 1884. It will be seen that the metacentre at all angles of inclination keeps on the same side of the C.G., and that therefore no intermediate position of unstable equilibrium occurs.

The metacentric height in upright position is 4·75 feet above C.G., and at 180° there is a negative metacentric height of 2 8 ft., making it impossible to balance even the model in this position. The results of the calculation are again shown in Fig. 3 (Plate xxxix.) in a manner easy to understand. This form of diagram, which has not been published before, has been proposed by Mr. Taylor, and is based upon the same theory as that of Mr. Benjamin. Instead, however, of setting off the heights from the point A, as in Fig. 2, a circle is used as base for the same, which has the advantage that the vessel, in its inclined position, can be shewn at different angles, at the same time giving the inclined waterlines and demonstrating the righting arms, thus depriving the study of the diagram of the confusing appearance which curves alone necessarily have.

It will be seen that the stability of the boat shews a very great margin of safety, and thus ample allowance is made for unforeseen circumstances. Moreover, although the boat has been designed to carry 70 passengers *inside* the cabins, even if this number were doubled by carrying another 70 on her deck, where they would have to hold on to the rails, the righting arms would be scarcely altered, and would remain positive through all angles.

The stability of the boat amongst waves is greatly increased by the enormous amount of surplus buoyancy, which is nearly 300 per cent. at the points where the openings of the boat are situated.

Watertight Divisions.

To make the vessel practically unsinkable, two sets of buoyancy chambers are provided, each set being of a cubical capacity exceeding the loaded displacement. One set is in the boat proper, at the ends and sides of cabins and engines room, and consists of 16 cells, which may be filled with cork; the other set in the deckhouse consists of three compartments. Thus the vessel consists of 22 watertight divisions. Each buoyancy chamber on the port side of the boat proper is connected to the corresponding one on starboard by a pipe with a cock, so that if desired, a local damage on one side of the vessel will not result in a list of the same. Should the whole lower boat be smashed, the upper buoyancy chambers will keep her afloat, the weight of the lower boat providing sufficient stability to enable sails to be carried; provision is made for the passengers to take refuge in the upper compartments in such a case.

The bottom of the top buoyancy chambers is at such a distance from the top, that in case the vessel should completely turn over (a case which her stability makes impossible)

at no inclination can any solid water enter the funnel or any of the openings of the vessel (*see* Fig. 3), while steam pumps are provided to get rid of small quantities of water shipped.

The vessel is to be constructed of steel on the longitudinal system; the wooden keels are strongly connected to the main longitudinal frames of the bottom, which also carry direct the weight of engines and boiler. Instead of life lines, steel steps, punctured with hand holes forming part of the construction, are placed outside between the waterline and the fenders. A rocket apparatus and all other fittings for saving life are to be fitted, also an apparatus for pumping oil on the water when approaching wreckage, &c. The boat contains a vapour bath for resuscitating half drowned people.

As auxiliary power, two masts with trysails are provided. The rudders, situated at both ends, are constructed on a novel principle and are non-projecting. Owing, however, to the patents not being in a complete state, we leave it to a later opportunity to bring the same before this Institution, stating only that the experiments shew that they answer fully and work easily and reliably.

With regard to the shape of the grooves, as shewn in the models, we are well aware that if the boat were designed for speed only, we should have to modify them; however, for the purpose of a life boat, other qualities equally important had to be taken into consideration.

DISCUSSION.

Captain The Hon. H. W. Chetwynd, R.N.: My Lord and Gentlemen, nobody in England can feel a greater interest in this Paper than the Lifeboat Institution which I have the honour of representing here to-day, and I can only say, my Lord, that the Institution hails as its best friends and allies those who try to do anything to improve the means of saving life from shipwreck, and I have great pleasure in enrolling among them the names of Mr. Benjamin and Mr. Taylor, because I am of opinion that they deserve great credit for the public spirit and energy which they have shown in working out the details of this invention, and for the amount of time and forethought that they have expended, and not a little money also, no doubt. I think we have every reason to thank them for their very ingenious and novel invention, but at the same time I really must ask them, if I should criticize it unfavourably, not to think that I am doing so from a jealous or captious spirit—far from it—it is

only with the hope that they will go on until they make it perfect. Before turning to the Paper itself, I must make what will appear to many a very extraordinary statement, but it is a perfectly true one viz., the question of the application of steam power to the propulsion of life boats, and how to apply it is a very large and a very open one. It is a question which has received and is still receiving the attention of the Lifeboat Institution, and it is one that yet remains to be answered, but I have no hesitation in saying that some of our experts—by which I mean some of our most experienced and gallant coxswains, who know what it is to go through breakers—are of opinion that it is utterly unpractical. However, I must pass on from that and turn to the subject of the Paper. I will take it at the beginning, and read the first paragraph in which Mr. Taylor asserts that there is a "wide-spread prejudice against steam as a motive power for life boats." To that I must really take exception, and certainly on the part of the Lifeboat Institution. There is no such thing to show that this is so. I may tell you that within the last two years the Lifeboat Institution have spent a considerable sum of money in experiments. They are still carrying out experiments, and only at their last meeting they decided to appoint a Sub-Committee to visit the Liverpool Exhibition to see what was to be seen there, and to reconsider the whole subject. I do not think that looks like a wide-spread prejudice against steam. Then turn for a moment to the second page, in which these gentlemen say, "As a life boat has to encounter forces of immense magnitude tending to capsize her, she must possess an extraordinary amount of stability"—(that of course is the case). "Lifeboats have been designed which possess self-righting power, but only very little initial stability, but such boats are unpractical, because, although they always tend to come back to their keel, slight forces are sufficient to turn them through a complete circle of 360°." To that, my Lord, I must take the greatest exception. Of the life boats which have been designed possessing self-righting power, the Institution has on the coast at this moment some 270. The boats have been launched, since there has been a self-righting boat for service, which of course means going to a wreck, in more or less violent weather, upwards of 4,700 times, and have saved very nearly 12,000 lives. Now there have only been 39 of those boats capsized, and out of those 39 boats which have been capsized there was no loss of life in 20 or 21. I consider, Gentlemen, that that, so far from being unpractical, is a very practical proof of the stability and value of these boats. If they were so unpractical, so unstable, and so ready to turn over, not only would they have done it much more frequently, but would the Institution find the gallant seamen and fishermen ready to man them day and night, and to go anywhere in them in any weather in the way they do? Would that be the case if they are what they are said to be? Undoubtedly not. Those figures refer to the whole of the boats. But let us go a little further—take to the large-sized boats only. If there are any life boats in the world that have the right to be termed uncapsizible boats, it is the large-sized self-righting boats of this Institution, because of the 22 of them that there are in existence, which have been launched 653 times for service and have saved 1,668 lives, not one of them has capsized. Whether they are practical or not I think may be shown by the tests to which our boats are put to show their stability in service. One report which was received the other day I have, and it says this: "I have examined the coxswain and the most intelligent of the crew, and they agree with me it is a splendid boat, and they would go anywhere in her." Many others have arrived at the same conclusion. I think before we could examine into the merits of this life boat it was necessary to clear the road, by,

if I may use a simile, extracting these false quantities from the proposition. Now to turn to the boat itself. The inventors describe it as the steam life boat, which they consider is best suited for general coast service, to be 56 feet long, 16 feet beam, with a draught of about 3 feet 6 inches and 3 feet, the depth of keel, with a weight of 37 tons. Now to begin with, the greater part of our life boats are launched from the beach. There are a certain number kept afloat, but very few, and therefore general coast service of a life boat, I am justified in taking to mean one that is to be launched from the beach. This it would be simply impossible to do with a boat of this size and weight.

Mr. J. M. H. Taylor: She is to be kept afloat.

Captain Chetwynd: Then a boat of 56 feet long with a draught of 3 feet 6 inches is more than is desirable for any life boat. Experience shows us, that even our longest boats with a draught little exceeding 2 feet, in going on to outlying sands, frequently strike in the trough of the sea, and a draught of 3 feet 6 inches would of course strike sooner, and often prevent the boat getting to the wreck. Besides that, if run on to the sands, as is sometimes necessary, it would entail their remaining in such deep water that they would be right in the force of the heavier breakers, instead of being within them as would be the case with the lighter draught. Then there is their great weight of 37 tons. Any boat of 37 tons laid alongside a wreck would as surely be wrecked herself by her weight striking the wreck as the vessel she was alongside of. Then as to the question of the strength of this boat, I should prefer leaving that to a practical shipbuilder—and I am not that, but I am a sailor. However, at any rate, we all know a little about it, and it appears to me very doubtful indeed whether screws worked in those chambers, with the tunnels turning down behind them in that way, would obtain any power at all. It would require great power to attain 12 knots in smooth water. I may now pass on to the form of the boat above. The boat is decked in a very peculiar manner. The boat proper is only 3 feet below the water and 9 feet above. The weights are placed principally in the lower part, and she has immense buoyancy above. For going into breakers that lower hull has not, I believe, sufficient buoyancy to make it rise well to the seas. We all know the force of a breaker in a breaking wave to be enormous, and it would come down on that deck and that house with such force that no life-boat man could live there—I mean doing life-boat work among breakers. I believe I am hardly exaggerating in saying that at some time when they were forcing her against a head sea, they would even have those rails torn off. I believe she would be steady—I do not think she would roll much, but with her shallow draught, and her great height above water, added to that very great length, she would offer enormous resistance to the wind, and if she was going broadside on to it she would be driven away in all directions, at any rate she would be completely unmanageable in the breakers from the enormous power which the wind would have over her. Again, details are not sufficiently given to show what it would be, but in a vessel of that form, that dome or superstructure must be immensely strong, because the whole force of the breakers would come on that, and, if it were not immensely strong, would break it in. That would all add to the weight. Now in one case the inventor speaks of carrying 70 passengers on the deck. Well, I should be very sorry to be one of them, because I believe if there was any sea at all it would soon wash off the whole of them. I believe it is utterly

impossible for a life-boat crew to work on that exposed deck. Then with regard to the scientific part—the righting power—I think I may safely leave that to abler hands than mine, but one point which has occurred to me, in studying the diagrams which have been shown to me is this, if the boat has been thrown over by a heavy sea to an angle of 35°, which is anything but impossible, her deck will be under water, and when the deck is under water, not only will it wash anything off that is on it, but it would require a very great power to lift it and to bring it up again. I question whether it would do it. Then she is to be "fitted with a rocket apparatus and all other fittings for saving life." I do not understand for what purpose the rocket apparatus is necessary, because they could not work a rocket apparatus from such a life boat at all. The object of the life boat is to go alongside the vessel, and many of the men who are saved by life boats are in such a state that if they had a rocket apparatus, they could not use it. Then with regard to the apparatus for pumping oil on the water when approaching wreckage, that is a point, gentlemen, containing two questions—one question is the use of oil in breakers at all. That has been tested by extensive experiments which were carried out by the Lifeboat Institution and the Life-Saving Service of America at the same time; the two came to identically he same conclusions, although they had no communication with each other. Neither knew the other was drawing up a report, and it is not exaggerating at all to tell you that the report might have been drawn up by the same person. It was to the effect that in really heavy breakers that would affect a life boat, oil was useless; but putting that on one side, if oil can be used in breakers, it is useless to discharge it from the vessel to be protected. The oil, to be of service, must be discharged at a certain distance from the vessel to be protected in the direction in which the sea is coming, so as to give it time to spread and operate on the sea, before the sea which it is intended to disarm reaches the vessel. As I said before, the Lifeboat Institution has no end to serve except the saving of life from shipwreck, and it is only too glad to have any help from anybody it can. I must apologize for having troubled you so long, but I really felt I had no other course with regard to this Paper of Messrs. Taylor & Benjamin than to criticize it in the way I have done.

Admiral I. R. Ward: My excuse, my Lord and Gentlemen, for intruding on your time, which is now drawing rapidly to a close, is that, having been for 31½ years Chief Inspector of Life Boats to the Royal National Lifeboat Institution, I have necessarily very considerable experience of the practical part—the seaman's share—of the management of life boats in heavy, broken seas. Of steam, as applied to life boats, I have, of course, had no actual practice, because it has not yet been introduced. In the first place, I endorse the greater part of the remarks made by Captain Chetwynd, as to the character of this particular boat designed by Mr. Benjamin. I am not quite clear, being somewhat deaf, whether he sufficiently exculpated the Committee of the Institution from the charge of prejudice against steam life boats which Mr. Benjamin has made, and which I regret he should have done, and, as a Member of the Committee, it is a point on which I am naturally sensitive. No doubt that charge was made without any intention to hurt the feelings of Members of the Committee of the Institution, but being the only body that places life boats on the coasts, it is the only one to which the charge can be applied. The Committee of the Institution, from the very first I may say, has felt, and naturally felt, the great responsibility it was under, remembering that the lives of the thousands of boatmen who work its

life boats, and more indirectly the lives of the wrecked people whom they attempt to save, were at stake, to endeavour as far as they could to see that the best means were being provided for their safety. That responsibility the Committee always felt to be far too important a matter to allow any paltry prejudices to stand in the way of obtaining, and acting on the very best information that could be obtained. On the general question of steam life boats, I have considered the matter, I might almost say, thousands of times, and I should be glad to arrive at the conclusion that such life boats, so far as those inventions have yet reached, were practicable, but I have not yet been able to arrive at that conclusion. I may state, with regard to that question generally—not as to this special life boat—that amongst the reasons, we have thought there would be too great risk run, even in the necessary experiments which would have to be made before we could trust men's lives in such boats, because, be it remembered, that before any life boat is adopted it must be known to be a boat which can be thoroughly trusted. It would not be sufficient to send it out once or twice, but it should go out in the very heaviest seas on our coast under the most dangerous circumstances—circumstances under which experiments could not be conducted without serious risk of loss of life to every person on board. That alone, I think, would be a sufficient reason for the Institution to hesitate, unless they thought they were on very safe ground, in risking men's lives in making such experiments. Perhaps, however, the chief objection which the Institution has felt to steam life boats has been their great liability to derangement of their machinery. It has been my painful duty during my 31 years' service to have often had to throw cold water on the plans of inventors, with whom I, nevertheless, felt every possible sympathy, owing to their not having had the means of acquiring the practical knowledge which is so essential and so very special and technical in its character, as that of life boat work. Unless engaged in life boat work, no inventor can have a sufficiently practical knowledge of the forces of the sea and the requirements of life boats to enable them to apply their science and skill in perfecting their inventions. For, be it remembered, a life boat is not merely such a boat as will live in fine weather or a moderate sea, but it has to encounter weather and seas which would be fatal to any other boats. I have been several times myself, in boats of 32 and 33 feet length, almost perpendicular with the sea, and for a moment my thought has been that we were going to be thrown, as sailors would say, end over end. We have had a boat 32 feet long thrown end over end by the height and magnitude of the sea, and therefore everything which you can determine by your study with regard to stability, centres of gravity, metacentres, and such like scientific terms, which I, perhaps, but slightly understand, however you arrange them, from the mere force and magnitude of the sea are set at naught, and a boat of any description is as much at the mercy of the waves as a cork in the water, or as a little model of a foot in length, which would be tumbled over and over, and the sea would laugh at all your scientific deductions. Another reason why the Committee have hesitated to risk the lives of men in such boats has been that, if by the breaking down of their machinery or other cause they come to grief, they would be absolutely helpless. That vessel, the model on the table, by the force of a violent shock, even a shock alongside a wreck, which Captain Chetwynd has referred to, might have her whole machinery so deranged that she would not be able to do more than float helplessly to the shore—possibly to her own destruction and that of her crew. There are other questions for consideration, but time forbids my going into them Such vessels would require at every station a practical engineer

in pay, who might not be employed through the whole year, and who, perhaps, at the very moment he was wanted, might be sick or out of the way, and your boat would be without an engineer or anybody to work it. As regards this special boat, Captain Chetwynd has referred to the great hold the sea would have upon her. In the case of a heavy sea breaking over an ordinary life boat in the safest position, *i.e.*, stem on, a heavy, curling sea breaking over the boat will often fall down in her stern sheets, and the men in the safest position would be those in the bow. Such seas have carried men out of the boat over the stern. What would such a sea do, taking a boat like this model, broadside on? The men would be crushed! I remember in 1852, when I first went out in life boats, the fear of the fishermen, when I asked them to row into a very heavy, broken sea, as expressed by them, was, "Sir, if you take us into seas like that, they would break the backs of any one of us." That was their fear. What would such seas do with a boat like this one? A common sea will put four tons of water in a boat. Imagine four tons of water being hurled over the crew against such a structure as that! There is another point which Captain Chetwynd did not allude to, and that is, how are you going to take a wrecked crew on board such a vessel as this? There are two openings, or shafts I might call them, and if all the passengers did not simply slip off the top of the rounded roof and go overboard, the majority of them, I believe, would do so. We have often to take on board men who drop down from the bowsprit or jibboom end, or a leeyard arm, and how could you get them into small holes on the round surface of a deck like that? You could not get them into the boat, and that I look upon as a very serious objection. If it were practical to have such a vessel at all, you would have to give up all the self-righting part. Supposing she had 36 or 37 people only aboard of her, and she was thrown keel up——

The President: I am afraid you have occupied as much time as is allowed.

Admiral Ward: I am sorry to have had to make any disparaging remarks, because one has only to look at this model to see what care, thought and money has been bestowed upon it; but I am afraid Mr. Benjamin has not a sufficient knowledge of what a life boat has to go through, or he himself would be disheartened.

Mr. F. K. Barnes: My Lord and Gentlemen, I should like to say just a word or two on the subject of this Paper. I saw it at an early stage—before it was printed—and my first impression was that it was not a Paper that ought to be read in its present shape in this Institution. It is really a proposal, having only been worked out on paper. I thought the subject was hardly in a condition for a Paper to be read on it here now. Seeing, however, that the Paper has been read, I should like to say that the authors propose some very excellent things—their ideas are uncommonly good, but they want clarifying, and their designs must meet all the requirements of the special service. All I would now say is, that I am glad the Paper has been read in this Institution, and that we have had the advantage of hearing it discussed by Admiral Ward and Captain Chetwynd, who know so well the requirements of life boats. It is really for the naval architect to work out what is wanted. I believe now, that the inventors of this boat, who have had the benefit of the criticism of

the gentlemen above named, will go away from here and modify their ideas very considerably, and probably a great amount of good will result from the discussion that has now taken place. I can only say again, I am very glad the Paper has been read.

A Visitor: My Lord and Gentlemen, I had the honour of receiving an invitation to come to this Institution, I believe in consequence of the circumstance that I have had the gratification of spending some five years of my life, doing what little was in my power, for the sake of the preservation of life from shipwrecks, on the coast of this kingdom, and I felt, considering that I meet with the men who man the life boats, and considering that I am continually on the various parts of the coast, it was my duty in point of fact to come and make a few remarks. I must say that the first thing which struck me on looking at this Paper was the heading to it, which was first alluded to by Captain Chetwynd. He alluded to the widespread prejudice against steam, as a motive power, for life boats. Now, I find that everywhere, nearly along the whole of the coasts, they are crying out for steam to be applied to life boats. I find the cry is, we want more powerful life boats, and especially we must have something stronger than human power to carry those life boats. Now, I will show you an instance of the advantage of steam over other motive power. A little time ago I was at Deal, and there was a shipwreck occurred there, I think a little to the leeward of Kingston, but to the leeward distinctly of Deal. The Kingston boat went out, the Deal boat went out, and the Ramsgate boat went out. The boat which was taken out by a steam tug, and, therefore, which had some power stronger than human power, was the boat that rescued that crew from the wreck. Now, you understand, Gentlemen, perfectly well, that it had a very much greater distance to go than the other boats. One of those boats also, the Deal boat, was beating back, and I found that she was trying to come to the place where I stood, which was her proper home. She did not make the point. She was then under sail—by, I should think, 200 yards; I do not like to exaggerate, but I think it was something like 200 or 150 yards. In trying to make this point she certainly had to make a tack out, of more than three-quarters of a mile, before she could make the point at which she wanted to arrive. Of course I am alluding to boats that are manned by boatmen of the very highest class that we have in the whole kingdom. I asked them the reason why this boat had to go three-quarters of a mile to make a tack to make 200 yards about, and the answer was that a boat like that must make such an enormous amount of leeway, and, that, therefore, you had to go this extreme distance. Now, my Lord, I would most particularly draw a moral from that, and it is this, that a vessel when it is struck sometimes breaks its back, and goes down within a very short space of time indeed; and if it happens to be in such a position that the boats we have, for instance under sail, require to go three-quarters of a mile to make an advance of only 200 yards, that is death to the crew very frequently. You see, therefore, how very necessary it is that we should get a larger amount of motive power than any human power that can possibly be brought to bear. I think the next point I remarked here was this—and the same point was alluded to by Captain Chetwynd. It says in the Paper, life boats have been designed which possessed self-righting power. I am very pleased to see in the model that we have got here that it has this—misfortune I call it—of having the self-righting power, but it is deficient in one thing, if I might be allowed to criticize the boats of the National Lifeboat Institution, that is

with regard to being self-discharging. I will not attempt to state my own particular ideas, but I am quoting the opinions of boatmen that I have been in conversation, and in company with, and they say that boats that lift out of the water and have self-righting power, or both of them, at any rate sacrifice some of their stability by doing so. With regard to the model that we have here, I must say that I should like to see that particular part that is intended, I suppose, for self-righting to be done away with. I think that is an evil, and if, under the circumstances that have been alluded to, you had the fire put out, and she had to take the auxiliary power she has from these masts, I think she would make a considerable amount of leeway, which, on a lee shore, might bring her to grief. I think with regard to the question of the self-righting power it is rather a disadvantage to her, and I should like to see a steam life boat brought forward, that had sufficient buoyancy and safety, without this self-righting power attached to it at all. Captain Chetwynd introduced a great number of figures about the number of lives saved. I am delighted to know that the Lifeboat Institution does save a great many lives, and this is not the occasion when I shall call in question at all the figures which he has put before you, but I will tell you one thing that I know, and it is that if we look back to something like 25 years ago, we shall find that the loss of life on our coasts was something between 700 and 800 a year. Now, if you look at the loss of life that we have at this moment, with the increased number of life boats, and taking at the same time (because I do not wish to exaggerate) the very much increased amount of tonnage that we have on the coast, and, therefore, naturally we should suppose for that reason that we should have a greater loss of life, yet I am very grieved to think that the loss of life has increased to between 1,000 and 1,100 every year, with the exception of last year when there was a very great reduction, but I am sorry to say it has very much increased again this year, because I see from the report that the shipwrecks at any rate are now nearly double that which they were last year.

Captain Chetwynd: No, no.

The Visitor: A gentleman says, no, no. I only had the Official Report about three weeks ago, and it gives the total of shipwrecks. I am speaking only in round numbers, because I have not the details here, but as far as I can remember, there were as nearly as possible double the amount of shipwrecks this year that there were last.

The President: You will pardon me if I ask you to confine yourself as much as possible to the subject under discussion.

The Visitor: The only excuse I can offer for departing from the subject is that your Lordship allowed a little licence to Captain Chetwynd, and I am obliged to do it to a certain degree in answering his remarks, although I do so with the greatest deference. I do not think it is very necessary for me to say more, but I think that the figures I have shown you are sufficient to prove to you that we require to have a greater power, at any rate, than we have got for the purpose of saving life. I may say that I receive letters to this effect, nearly every day of my life, and I have here in my pocket, this morning, a letter of that particular kind which I have received. Therefore,

although the National Lifeboat Institution has done some good, and altogether a very large amount of good, yet I think that there is room for very great improvement, and I am perfectly satisfied of this also, that the public are beginning to see that we must not have the crews exposed quite so much as they have been exposed in some of our open life boats, and the model that we have before us, at any-rate, has the advantage, that they are in shelter, and will not be so much exposed. There is another feature which I have noticed about the boat we have here, and it is this, that there is provision made for people who are exhausted with cold and fatigue—something in the shape of a warm bed. It has been my sorrow to meet with cases, where the weaker vessels, the women and children, who have been rescued from shipwrecks, have been lost from exposure following after the shipwreck. Therefore, there are, at any rate, certain good points. I have not the slightest prejudice in favour of one boat or another in any shape or way. There are some very good points in this boat which I hope may be further developed, and I certainly desire most ardently to see the day when there shall be a greater power than human power given to life boats, and when we may see these sad figures diminish instead of being increased as they have been during the last 25 years.

Mr. G. Benjamin: My Lord, in replying I may first of all be allowed to express my regret that, owing to the extreme lateness of the hour, we have been deprived of an opportunity of having the Paper properly discussed by naval architects, as we had hoped it would be. Of course, it is very desirable that such a boat as this should be discussed by those men who have to man it; still, there are very many points in it, departures from principles of naval architecture, which must be left in the hands of the naval architect, just as the constructive part of a house is left in the hands of the architect, and not of the inhabitant, though his wishes may be duly considered by the architect. I am sorry to say that our Paper from this point of view has not received proper treatment. I am bound to express my gratification at the courteous tone in which Captain Chetwynd, and after him, Admiral Ward, spoke of us, although I could have wished that they had drawn the line a little clearer between an invention and a carefully considered design. Captain Chetwynd finds fault with a great many things in our design, more than I shall have time to reply to. But if you come to consider them, piece by piece, you will find they are all minor points, and you will notice that against the principal points his criticism has not been directed. Captain Chetwynd does not say that steam is not the proper motive power for a life boat—on the contrary, I understand from his remarks that he wants a proper steam life boat.

Captain Chetwynd: Hear, hear.

Mr. Benjamin: And that remark of Captain Chetwynd found approval by many of the members present. Regarding now the design we have laid before you, there is one remark of Captain Chetwynd, which, on the first glance appears, to be important, viz., the weight of the boat. The displacement of 36 tons, as stated in the Paper, is that of the boat fully loaded with passengers, coal, stores, &c. The weight of the boat and machinery is only 28 tons, and that is not so much more than a common life boat.

Captain CHETWYND: No, no; the figures are——

The PRESIDENT: The discussion is now closed, Captain Chetwynd, and Mr. Benjamin is replying.

Mr. BENJAMIN: I say that 28 tons is not such an enormous weight. If you want steam you must have an increased weight, there is no doubt about that. Besides, we advocate keeping the boats continually afloat, and then the difficulty of launching the increased weight is done away with. Captain Chetwynd makes a mistake if he applies the introductory remark of our Paper that there is a wide-spread prejudice against steam life boats, as directed against the National Lifeboat Institution, and the same in regard to the remark on the second page of the Paper, that life boats have been designed which possess self-righting power, but not sufficient initial stability. In the latter remark we speak of designs of steam life boats made outside their Institution, and I never for a moment imagined, neither did Mr. Taylor, that the prejudice against steam life boats would have been shared by such an enlightened body as the National Lifeboat Institution. Captain Chetwynd spoke of the position of the screws in our design, but just on this point, more than any other, I should have desired to have heard the opinion of the authorities on this question. As I remarked to-day in the discussion on the Paper of Mons. Marchal, we have made special investigations in this direction, and I believe we shall be able to carry these investigations so far as to show that this position of the propellers, which is very desirable from many considerations, especially in a boat like this, will not only not offer any hindrance to speed, but can, if proper care is taken in the design of the vessel, be made conducive to the attainment of perhaps higher speeds than any other position of the screws. Of course this is at present only a proposition, but we mean to endeavour to carry this proposition out in practice. However this may be, Captain Chetwynd will not be offended if I say that this is a question for the naval architect, and for the naval architect *only*. Regarding some minor points of the discussion, such as, that the rails will be washed off, that the tophouse cannot be constructed light, and at the same time strong enough, to stand wind and waves, that no one would go into the hatchways of the boat, &c., I am sorry, my Lord, that the time does not allow now to reply to these objections, and as it is too late to extend my remarks any further, I shall only refer to one question more. Admiral Ward has expressed his disbelief in metacentres and stability calculations. All vessels built on novel principles have their stability calculations made out, and the captains must trust that the naval architects know what they are doing.

The PRESIDENT: Perhaps, Gentlemen, as I have stopped Captain Chetwynd, he might now give you the figures that he proposed to give when I interrupted him, because I did not wish to introduce a controversy when a gentleman was speaking in reply.

Captain CHETWYND: I am much obliged to your Lordship. The weights of the life boats differ with the size. The ordinary coast life boat weighs from $2\frac{1}{2}$ to $3\frac{1}{2}$ tons. The largest boats in the service, of which there are about four altogether, weigh about 10 tons—everything on board—and in that I may say I think myself, and it is the general opinion, we have nearly reached the limit of possible weight for life boat work.

Mr. J. M. H. Taylor: My Lord and Gentlemen, the time is so short I am unable to reply to the gentlemen connected with the Lifeboat Institution as I should have liked, and will, therefore, make my remarks as brief as possible, at the same time thanking Captain Chetwynd for his courtesy, and will only say that it seems to me remarkably strange that in these days of engineering enterprise steam has not been successfully applied to life-saving vessels—to say nothing of the property that might be saved—and I fail to see any reasons whatever why steam cannot be applied to this class of vessel. Mr. Benjamin and myself have done our best to produce something like an efficient steam life-saving apparatus, and even if our design be not accepted and adopted by the Lifeboat Institution, but is the means of bringing a better design to the front, we have gained an object in the right direction.

The President: Gentlemen, I am sure you will allow me to convey your thanks to Mr. Benjamin and Mr. Taylor for exhibiting this boat, which I hope will prove an important step forward in a great purpose which we all have at heart. I am perfectly certain that no subject could be brought before this Institution which could interest us more deeply than the saving of life at sea by means of the life boat. The use of steam in life boats is a subject which has been very much discussed, and it does not appear at present to have made much way; but I do not think in reference to this discussion, which, though short, has been a very valuable one, we could possibly have commanded the services of two contributors to the discussion more valuable than those of the two gentlemen who have passed their lives, or I may say are passing their lives, in the life boat service of this country, Admiral Ward and Captain Chetwynd. I am sure our united thanks are due both to Mr. Benjamin and Mr. Taylor, and likewise to those two gentlemen who have given us the result of their large and valuable experience.

ON CONVERTING EXISTING COMPOUND ENGINES INTO TRIPLE-EXPANSION ENGINES.

By H. A. B. Cole, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; Admiral the Right Hon. Sir John Dalrymple Hay, Bart., K.C.B., D.C.L., F.R.S., in the Chair.]

It is proposed, in the following Paper, to discuss the conversion of existing compound engines with two cranks, into "triple-expansion" engines, by means of the addition of one or two cylinders to be fitted above the present ones, thus making one or each of the engines in any particular case a "Tandem." It is considered that the advantages of a high working pressure can thus be secured, at any rate in a great many cases, by a comparatively inexpensive alteration which it is thought will recommend itself to shipowners when renewing boilers, as being preferable to putting in new engines of the three-crank type, unless, indeed, it can be shown that this latter type of engine is much more economical in *transmitting* the Indicated Power than a two-crank engine.

In projecting an alteration of this kind, the following considerations are of primary importance, viz. :—

1. The initial loads on the pistons must be kept within the limits of strength of the piston-rods and connecting rods, and they should not, when the engines are working, bring too great a pressure upon the crank-pins, in order that the lubricant may not be squeezed out of the bearings. This consideration also leads to the desirability of the initial or maximum loads on both crank-pins approaching equality, when the engines are working.

2. The total I.H.P. should be about equally divided between the two engines (not necessarily between all the cylinders) in order to attain the smallest ratio of maximum to mean combined twisting moment.

These conditions are to a certain extent antagonistic, and a compromise must therefore be adopted so as to combine them as well as possible in each case.

The first point, viz., that referring to the maximum load upon the rods and crank-pins, is particularly important, as it is evident that, in some cases, this consideration will limit the working pressure of the new boilers.

The method now suggested of arriving at a conclusion as to the conversion of a pair of engines, is to draw out an ideal expansion curve from results already achieved in practice, and thus to take account of the condensation which actually takes place during expansion, the amount of which condensation can only thus empirically be arrived at, and then to subdivide the area of this ideal expansion curve in such a manner as to give the most favourable distribution of work to the three (or four) cylinders, with reference to the conditions just mentioned.

Some investigation of the working of triple-expansion engines shows, that if a curve (Fig. 1, Plate XL.) of the equation $p_1 v_1^n = p v^n$, in which the volumes include all clearances, and the pressures are absolute, be assumed to circumscribe or contain the ideal "cards" corresponding to the several cylinders, so that their expansion portions coincide with it, this curve will be a very fair guide to what will afterwards take place in practice. The value of the index n is arrived at by actual experience, and will in general be about $n = 1{\cdot}2$, varying a little with the total range of expansion, and also varying according as the cylinders are jacketed or not. The whole investigation can pretend to nothing more than to lead to an approximation sufficiently accurate for practical purposes.

In getting out the expansion curve, the use of "sectional" paper will save much time and trouble. If there are any trustworthy indicator cards of the engine which is to be altered, and if the total I.H.P. is to be kept about the same as before, the mean pressure of the I.H.P. reduced to the L.P. piston is to be taken as the basis of calculation, but if it is not known, 23 to 25 lbs. is a good approximation to work with. This pressure is then multiplied by a factor, also determined by empirical investigation, and found to vary according to the amount of clearance and loss of pressure between the cylinders from 1·28 to 1·36. This product is to be taken as the mean pressure of the expansion curve.

A curve (Fig. 2, Plate XL.) of cut-offs and mean pressures has been calculated for the index $n = 1{\cdot}2$ (similar to Professor Rankine's for the "saturation curve," in which the index is $\frac{17}{16}$, or 1·0625).

Having provisionally decided upon the working pressure of the boilers, and allowed

for loss of initial pressure in the new H.P. cylinder, due to wire-drawing, we can, from this curve, at once find the cut-off, or proportion of $\frac{V_1}{V_2}$ which will give the required mean pressure.

At this stage it is necessary to calculate the terminal pressure, so as to ascertain whether it falls below a workable limit. In some cases the terminal pressure in the original unaltered engine may be unnecessarily high, so as to admit of a reduction; in others, where it is already low, the initial pressure first chosen may have to be reduced, or a liner fitted in the L.P. cylinder.

Having thus fixed upon p_1 the mean pressure of admission in the new H.P. cylinder, and v_1 the volume at cut-off (including the clearance) in the same cylinder, we can proceed to trace the expansion curve from the equation. The pressure p in lbs. per square inch absolute for any abscissa v in cubic feet is found by the relation

$$\log p = \log p_1 + n \log v_1 - n \log v.$$

n being here taken as $n = 1{\cdot}2$.

The value of $\log p_1 + n \log v_1$ having been once determined for each case, is a constant, and the values of $n \log v$ are given in the table in the Appendix A for values of v ranging from 1 to 50 cubic feet. The base line of the expansion curve is the total volume of the L.P. cylinder, that is, including clearance and deducting the volume of the rods. Of course the more accurately the clearance space of each cylinder is known the better.

The expansion curve being thus drawn, its area is to be apportioned between the cylinders, as before mentioned. The clearance volume and working volume of each of the two original cylinders are to be marked off at their corresponding abscissæ. The desired sub-division of the area of the curve can only be accomplished by "trial and error." The initial pressures in the two original cylinders must in the first instance be chosen and an allowance of say 2 to 3 lbs. made for loss of pressure between the cylinders, as well as 4 lbs. for back pressure in the L.P. cylinder. The volumes of the two existing cylinders are of course given, but the volume of the new H.P. cylinder has to be found. It is at once provisionally determined by choosing an admission pressure for the intermediate (formerly H.P.) cylinder and drawing a horizontal line 2 to 3 lbs. above this admission pressure. This line will obviously cut the expansion curve in the point corresponding to the total volume of the new H.P. cylinder, and its clearance must be set off in the same way as that of the other cylinders.

In cases where two equal cylinders are to be added, each receiving its steam direct from the boilers, this volume of course represents the sum of the volumes of the two new cylinders. The question as to whether it is better to adopt two new cylinders, with or without quadruple expansion, or one new cylinder over the original H.P. cylinder, or one over the original L.P. cylinder must be decided on the merits of each particular case, and depends of course chiefly on the ratio of the volumes of the original two cylinders.

Having settled upon a "trial" volume for the new H.P. cylinder and drawn in the admission, exhaust, and compression lines of the three cards by experience, it will lead most rapidly to a solution of the problem to first of all check the total indicated horse-power, as the factor, with which the original pressure of the I.H.P. reduced to the L.P. piston is multiplied in order to determine the mean pressure of the expansion curve, is but an empirical one. The total I.H.P. does not vary much (within reasonable limits) however it may be subdivided or shared between the cylinders. These trial cards represent the mean between the "top" and "bottom" cards of the engine after the alteration, and the I.H.P. can be calculated from them in the usual way, the number of revolutions having been determined upon at the outset. Should the power come out too high or too low, then the cut-off volume v_1 in the new H.P. cylinder must be altered and a new curve traced until the desired power is arrived at.

The work in the two engines can then be equalized by adjusting the three cards in such a manner as to give the required mean pressure in each.

The next operation is to ascertain the initial loads on the piston rods and crank-pins, and find whether they are within the limits of strength of the former, and the proper working pressure for the bearings of the latter, say about 450 lbs. per square inch. It is scarcely necessary to observe that the "load" for any point of the stroke is found by measuring the pressure from the top of the card corresponding to the stroke in question (either "top" or "bottom") to the bottom of the return stroke card, which may be called the "load pressure," adding or subtracting the weight per square inch of piston area of the piston and rods, for the "top" or "bottom" card respectively, and multiplying the pressure thus arrived at by the area of the piston in square inches. The load pressures become negative from the point where the expansion or release portion of the card cuts the compression line of the return stroke card.

The loads, thus ascertained, however, require to be corrected for the momentum of

the pistons, piston-rods, and connecting rods, as this materially modifies them, especially, of course, in the L.P. cylinder.

It is well known that the load upon the crank-pin at the beginning of the stroke is less than that which would arise from the load-pressure shown on the indicator diagram, by an amount due to the force corresponding to the accleration of the motion of the piston and rods. This acceleration diminishes till it reaches zero at 90° angle of the crank from either "centre" and then, becoming negative, brings a gradually increasing pressure upon the crank-pin during the second quadrant of the revolution, in which the piston and rods deliver to the crank the energy they absorbed in their motion becoming accelerated during the first quadrant. The method of calculating the effect of this acceleration is given in the Appendix B. on the assumption that the angular velocity of the crank is constant and the connecting rod infinitely long. It may be worth mentioning that, taking the weight of piston and rods at $3\frac{1}{2}$ lbs. per square inch of piston area (as is done throughout this Paper), the difference of pressure due to momentum of the piston and rods amounts at the beginning of the stroke for 67 revolutions and 36 inches stroke to no less than 8·1 lbs. per square inch, which in the low pressure cylinder is a very large fraction of the whole indicated pressure.

It must be borne in mind that the "cards" constructed by the method which is the subject of this Paper, represent the means of the "top" and "bottom" working cards. In subsequently setting the slides, of course the usual difference is to be made between the top and bottom cut-offs, &c., so as to approximately balance the effect of the *weight* of the piston and rods. The effect of their *momentum* alone is the same for top and bottom, or as if the engine were horizontal. In calculating the twisting moments from these ideal cards, this difference between the top and bottom loads must be allowed for. The loads, as thus corrected for momentum and weight will still be somewhat in excess of the truth, as guide friction is here neglected as well as the resistance of the pumps, which also affects the result to some extent, except in cases where they are driven either by an independent engine or by excentrics on the main shaft.

The diagram (Fig. 1) represents the case of a pair of engines 28″ and 50″ × by 36″ to be altered by adding one new H.P. cylinder above the present L.P. cylinder. The I.H.P. of the original engine was 595 on trial trip, with 67 revolutions and 80 lbs. working pressure. The new boiler pressure is purposely chosen very high (so as to give a somewhat extreme example), viz., 195 lbs. Allowing 20 lbs. for wire-drawing, we have 190 lbs. mean absolute pressure of admission.

The work has been distributed to the three cylinders in three different ways, as follows, viz. :—

Case I.—Initial working loads on crank-pins equal (dotted lines).

Case II.—Shares of work done by both engines equal (dot and stroke lines).

Case III.—Compromise between the above two cases, the initial pressures in M.P. and L.P. cylinders taken midway between the former ones (full lines).

The particulars of power, loads, twisting moments, &c., for these three cases and for the original trial-trip conditions of the engines are given in the Table on page 324. Fig. 3 (Plate XLI.) shews the twisting moments.

The after engine, or original L.P. engine, is the "leading" one. It will be seen that the ratio of maximum to mean twisting moment is the same in Case III. as in the original engine, also that the *standing* loads in this Case (III.) are very nearly equal. The loads for the twisting moments of the original engine are of course corrected for weight as well as momentum; those for the other cases have been measured from the ideal cards (which, as before stated, represent the mean of top and bottom), on the assumption that the cut-offs are so arranged that the weight of the piston and rods is just balanced in the new high-pressure cylinder, or, in other words, that the mean pressure of the bottom card of this cylinder is 7 lbs. in excess of that of the top card, and that this difference of pressure applies in the M.P. and L.P. cylinders to the admission portion of the cards only. Of course, this is to be understood as an approximation to what would really take place.

The diagrams of twisting moments of two successful three-crank triple-expansion engines are given on Fig. 3, the cranks of which are placed 120° apart. The object of inserting these diagrams, is to shew that it is not only possible in altering a two-crank engine to get the ratio of maximum to mean twisting moment as low as in ordinary two-crank compound engines with cranks at 90°, but also to make it nearly as favourable as that of three-crank engines. This ratio in Case III. is 1·69 and in the three-crank engines it is 1·47 and 1·446 as shewn. The effect of weight and momentum of the pistons and rods has been taken into account in all these twisting moments.

The working pressure being taken very high, the whole process of the alteration here described as a specimen may be looked upon as approaching a limiting one, as it is evident that the difficulty of keeping the initial loads and ratio of maximum to mean twisting

	Case I. Dotted lines.	Case II. Dot and stroke lines.	Case III. Full lines.	Original Engine.	
				Top.	Bottom.
Mean pressure of admission, absolute, lbs.	190	190	190	86	91
Ratio of Expansion	20·4	20·4	20·4	8·62	
Mean pressure of Expansion Curve, $p\ v^{1·2} = p_1\ v_1^{1·2}$	30·4	30·4	30·4		
,, ,, ,, I.H.P. reduced to L.P. piston	23 65	23·5	23·51	25·08	
Ratio of these two pressures	1·285	1·294	1·293		
Total volume of new H.P. cylinder, cubic ft.	4	3·566	3·75		
,, ,, Intermediate ,,	13	13	13	13	
,, ,, Low ,,	43·11	43·11	43·11	43·11	
Diameter of new H.P. ,,	15½	14⅝	15		
I.H.P. in ,, ,, ,,	156	122	137		
,, ,, Intermediate ,,	221	283	254		
,, ,, Low ,,	189	157	173		
,, of Forward Engine	221	283	254	334·8	
,, ,, After ,,	345	279	310	260·7	
,, total	566	562	564	595·5	
Initial load of Forward Engine, working, tons	15·07	20	17·54	16·2	16·81
,, ,, ,, ,, ,, standing, ,,	17·14	22·2	19·89	18·5	19·05
,, ,, ,, After ,, working, ,,	15·5	10·17	12·5	7·45	1·75
,, ,, ,, ,, ,, standing, ,,	22·96	17·71	20·12	14·5	8·76
,, ,, ,, Forward Engine, working, per sq. in. of crank pin lbs.	364	484	424	394	406
,, ,, ,, ,, ,, standing, ,, ,,	385	497	446	447	460
,, ,, ,, After ,, working, ,, ,,	375	246	302	185	42
,, ,, ,, ,, ,, standing, ,, ,,	515	397	450	351	222
,, ,, ,, Forward ,, working, per sq. in. of piston rod body, lbs.	1558	2065	1810	1680	1735
,, ,, ,, ,, ,, standing, ,, ,,	1770	2285	2060	1910	1960
,, ,, ,, After ,, working, ,, ,,	1600	1050	1290	768	185
,, ,, ,, ,, ,, standing, ,, ,,	2370	1825	2080	1495	905
Mean combined twisting moment inch-tons	230	227	230	239	
Maximum ,, ,, ,, ,,	358	405	390	405	
Ratio of max. to mean ,, ,, ,,	1·56	1·78	1·69	1·69	
Range of temperature in new H.P. cylinder	61·5°	51·5°	56·5°		
,, ,, Intermediate	83°	105°	93·5°	87·5°	
,, ,, Low	57°	63°	69°	75°	

moment within practical working limits increases with the working pressure, so long as only *one* new cylinder is used. It will be seen that the strains upon the piston-rod (body) are quite moderate, even when these are calculated without the correction for momentum, that is, when they represent the state of things existing with either engine stopped on the centre and full steam on. In practice, of course, the working pressure will seldom be so high as 195 lbs., as the extra cost of boilers for the last 20 lbs. or so of pressure is out of all proportion to the small piece of the top of the combined diagram, which corresponds to the work got out of them. The working pressure was purposely chosen thus high in order to show the limits to which this kind of alteration may be carried, without regard to the question of cost of boilers.

It thus appears that, in most cases, the alteration can probably be successfully carried out by means of the addition of only one new cylinder, which will generally be placed more advantageously over the L.P. cylinder than over the original H.P. cylinder. This last point is evident for several reasons. For if the new cylinder be placed over the M.P. cylinder, then the L.P. cylinder must do approximately half the total I.H.P., and this, considering the attenuation of the expansion portion of the L.P. card, leads to a very high initial pressure in the L.P. cylinder, which again, of course, entails difficulty with the load on piston-rod and crank-pin bearings. Further, by giving only one-fourth, or thereabouts, of the whole horse power to the L.P. cylinder, we are enabled to keep its range of temperature much lower, as just in this portion of the "combined diagram" the temperature rises with the pressure most rapidly.

Lastly, it appears that the best arrangement will generally approach that in which the initial pressures in the M.P. and L.P. cylinders are midway between those corresponding to *equal initial loads* (taking momentum into account), and *equal work* in both engines.

In order to avoid long passages, which would be required if the new H.P. slide were to be driven from an extension of the existing L.P. slide-rod, levers can be used, as in the case of the S.S. "Sobralense," constructed by the Barrow Shipbuilding Company. Messrs. Manuel and Marshall's patent stuffing-box can be very advantageously applied between the H.P. and L.P. cylinders.

In conclusion, I would only remark that this Paper is an attempt to show the limits up to which we can work with safety and advantage in converting existing compound engines, and to suggest the general lines on which the design should proceed.

APPENDIX A.

Table of values of $n \log v$ for constructing the expansion-curve of the equation

$$p\, v^n = p_1\, v_1^n \text{ for } n = 1{\cdot}2.$$

for $v = 2$, $n \log v$	= 0·361236.
3	0·572544.
4	0·722472.
5	0·838764.
6	0·933780.
7	1·014120.
8	1·083708.
9	1·145088.
10	1·200000.
11	1·249680
12	1·295016.
13	1·336728.
14	1·375356.
15	1·411308.
16	1·444944.
17	1·476540.
18	1·506324.
19	1·534500.
20	1·561236.
25	1·677528.
30	1·772544.
35	1·852884.
40	1·922472.
45	1·983852.
50	2·038764.

APPENDIX B.

The effect which the inertia and weight of the piston and rods has upon the "loads" at the various points of the stroke, is determined as follows.

We must first have an expression for the acceleration of motion of the piston for any angle which the crank makes with the "top and bottom centre line" or axis of the cylinder produced.

Then, assuming the connecting rod ∞.

Let r = arm of crank, in feet.

a = angle of crank.

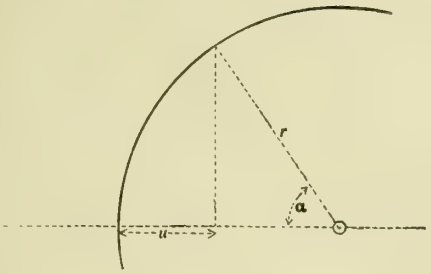
u = distance of piston from end of stroke.

t = time in which the piston travels through u.

v = velocity of piston.

ω = angular velocity of crank shaft, assumed to be constant.

p = acceleration of the motion of the piston.



By the laws of motion we have

$$v = \frac{du}{dt} \quad : \quad p = \frac{dv}{dt} \quad ; \; dt \text{ constant.}$$

$$p = \frac{d^2 u}{dt^2} \quad ; \quad \text{but} \quad t = \frac{a}{\omega}$$

$$\therefore d t^2 = \frac{d a^2}{\omega^2}$$

because ω is constant.

$$\text{then } p = \frac{d^2 u \, \omega^2}{d a^2}. \qquad \text{(I.)}$$

$$\text{now } u = r (1 - \cos. a) = r - r \cos. a.$$
$$d u = r \sin. a \, d a.$$
$$d^2 u = r \cos. a \, d a^2,$$

Substituting this in Eq. I., we get

$$p = \frac{r \cos. a \, d a^2 \, \omega^2}{d a^2}$$

$$p = r \, \omega^2 \cos. a.$$

It is evident that this value is a maximum at the beginning and end of each stroke, that it is positive at the beginning, equal and negative at the end and passes through zero at half stroke where $a = 90°$.

Having the acceleration (positive or negative) and the mass of the piston and rods, we can at once find the difference of pressure per square inch due to the acceleration for any angle a.

Calling this difference of pressure per square inch Q and the area of the piston A, and taking the weight of the piston and rods at $3\frac{1}{2}$ lbs. per square inch of piston area,

$$\text{we have the mass} = \frac{3{\cdot}5 \times A}{32{\cdot}2}$$

$$\text{and } Q = \frac{3{\cdot}5 \times A \times r\,\omega^2 \cos.\,a}{32{\cdot}2 \times A} = 0{\cdot}11\,r\,\omega^2 \cos.\,a$$

for the top or bottom centre where cos. $a = +1$ and -1, $Q = 0{\cdot}11\,r\,\omega^2$ where r = arm of crank in feet.

In the case of our engine, $r = 1{\cdot}5$, $\omega = 7$, Q is 8·08 lbs. per square inch.

For any other angle Q can be found by means of the following simple construction.

As the distance of the piston from the end of the stroke is (with an infinitely long connecting-rod) proportional to the cosine of the angle a of the crank, and as we have just seen, the value of Q is also proportional to the cosine of a, it is evident that these distances from the end of the stroke, and values of Q can be represented by a series of similar triangles.

Referring to Fig. 4 (Plate XL.), which represents the trial trip cards of the L.P. cylinder of our original engine, it will be seen that the value of Q for any point of the stroke can be very easily determined as follows:

Draw two straight lines A B., C D. parallel to the atmospheric line of the cards; make A B.= the length of the top card, and C D. = the length of the bottom card by drawing perpendiculars from the ends of the cards to cut the lines A B., C D. Then set off the value of Q for the beginning of the stroke upon these perpendiculars, taking A B. and C D. as bases, and using the scale of pressure which corresponds to the card, in the negative direction for the beginning of the stroke and the positive direction for the end of the stroke, and join the points E F. and G H. thus determined, by dotted lines.

Then it is evident, from the consideration of the similar triangles before referred to, that the heights between the horizontal lines A B. and C D., and the oblique dotted lines E F. and G H. express the positive or negative values of Q for any position of the piston.

The correction for weight is now to be made by setting off $3\frac{1}{2}$ lbs. on the perpendiculars in the positive direction for the top card, and the negative direction for the bottom card. If the points thus determined be now joined by the full lines, the whole correction for weight and momentum together, being the height between the horizontal line A B. or C D. and the oblique full line for any point of the stroke, can be taken off and transferred to the indicator card, thus giving the "load pressure" for any point. The result in our case is shewn by the dotted lines on the indicator cards, and it will be seen how materially these corrected "load pressures" differ from the pressures shewn on the cards. In calculating the twisting moments these corrected load pressures are to be used.

DISCUSSION.

Mr. A. E. Seaton: Sir John Hay and Gentlemen, I have been called upon to open this debate, but I am quite unprepared to do so. Last year I read a Paper myself on Triple-Expansion Engines, and alluded in that to some cases in which we had what is commonly called triple compounded an old engine. We have done so to one or two engines, and I quite agree with the author of this Paper that the low-pressure cylinder is the better one on which to place the third cylinder. The boat I then spoke of has been continuously working since this time last year, and has given uniformly good results. It was a case in which you will remember the same boilers were used after re-compounding as before, having been made for a much higher pressure than the old engines were capable of working at. In that case the new high-pressure cylinder was placed on the old low-pressure cylinder, and we did it for very much the same reasons as Mr. Cole has mentioned. In fact those particular engines had a much larger low-pressure rod than had the high-pressure engine. When we came to go into the question of strain, we found the high-pressure connecting rod was not capable of taking the additional cylinder. But there was another reason found after we had thrashed the thing out; we discovered that we might have saved ourselves a great deal of trouble, because, on referring to the plans of the ship, we found the galley came on the top of the H.P. cylinder, so we could not have put it there if we had wished. We have re-compounded several old ships by putting high-pressure cylinders on the original high-pressure cylinders, and we have found that, though we went very carefully into the diagrams, as recommended by Mr. Cole, we could not in practice divide the work equally between the two engines. We found it was a very easy and enjoyable thing to sit down in the drawing office and get out ideal diagrams and divide the powers and strains, but it was a very difficult thing indeed to get the actual diagrams to correspond with the ideal ones. There is no doubt, as soon as shipowners feel they are in a position to spend money on new boilers, many of them will adopt the higher pressures and re-compound their engines; but, at the present time, there is not the disposition to do it which one could desire as manufacturers. There are certain things which superintending engineers view with a stronger objection than we do ourselves, but there are valid objections to the re-compounding some of the old engines. I will not go into them to-night, as most manufacturing engineers know of them. A great many engineers have preferred to take the old two-cylinder engines out and put new three-cylinder engines in rather than to spend any money on an obsolete and, perhaps, a half worn out engine. Mr. Cole speaks of a pressure of 450 pounds to the square inch for journals. If that is his practice he must have more trouble than most people with them. Four hundred and fifty pounds per square inch is quite sufficient for the pin without putting that pressure on the journals, and although I know the tendency of the last few years has been to cut down the journals of triple-expansion engines until it is not at all an uncommon thing for some engineers to make the main journals each equal in length to the diameter of the shaft, and in some cases less, I do not think it is a good practice, and I think it is a retrograde movement. We are going to have a Paper to-night that will probably contain to some extent a cure for that kind of thing; but I think that prevention is better than cure, and in my own practice I go in for journals quite as long, or almost as long, as we did in the ordinary

compound engines, and the result has been that in one triple compound engine a hot bearing or worn brasses is an unknown thing. I may mention that, notwithstanding the difficulty and almost impossibility of dividing the work between the engines anything like equally, the engine itself works with a uniformity of angular movement that is quite equal, I think, to the best of two-cylinder engines with the cranks at right angles. The first engine we made on the triple system had the tandem cylinder, and I was congratulating myself on the fact that we succeeded in getting in the cylinders our ideal diagram, because the engine worked even at the very slowest speed uniformly, until I was disgusted to find when the diagrams were worked out that the power was nearly two to one. Now attempts have been made by some engineers to equalise the work by fitting an expansion valve to the low-pressure cylinder, and thus getting a very early cut off, but that involves consequences quite as serious, if not more so, than the original evil of having the greater work on the forward crank pin. The after crank has of course not only to bear the strain of its own piston, but it has to transmit the work from the forward engine, and for that reason I think we can with perfect safety and comfort be content with considerably less work on the after engine, inasmuch as the forward engine, as it has not to transmit anything but its own work, is not so strained as the after crank. The tandem engines I speak of have now been running continuously for about three years, and though at the outset there was a little tendency for the forward brasses to wear more rapidly than the after ones, we have got over that now, and have no complaints whatever. I quite agree with what Mr. Cole has said in one short paragraph that is in my memory, and that is of the advisability of adopting such a plan as Mr. Manuel's, between the high pressure and the medium pressure cylinders. The high-pressure gland from the heat of the steam and the deposit of water, and the medium pressure gland from the absence of lubricants, as is often the case, gives trouble, and it is a difficult thing then to keep them tight and in good working order, but by the substitution of Mr. Manuel's ingenious apparatus I think that will be entirely obviated, and so a step will be made in the right direction. I was very pleased to find it was mentioned here to-night. The saving in fuel with what some engineers call the long and short leg engine, that is, with the power unevenly divided, is very satisfactory. There is very little difference, if any, for I.H.P. between it and the three-crank engine ; the propelling effect, however, I do not think is nearly so good. I am so convinced of that, that even in the very smallest engines we have been making lately I have preferred to go to the expense of making them three-crank rather than tandem. I do not think there is anything more which I can say on the subject, as it has come upon me quite unprepared.

Mr. J. Macfarlane Gray : In Mr. Cole's Paper, the equation of the approximate adiabatic expansion curve is given as $p^5v^6 =$ a constant. This differs considerably from the $p^9v^{10} =$ constant, given by Rankine, also as an approximation. I had occasion lately to set out the adiabatic curve for initially dry steam at 100 pounds gross pressure expanding to 3 pounds. I then found that the curve, $p^6v^7 =$ a constant, was almost identical with what I obtained. This independent evidence therefore corroborates the indices given in the Paper, although these differ so much from what have generally been employed. Mr. Cole's method of dealing with the effect of the inertia of the parts in vertical movement is unnecessarily tedious, and it leaves out the influence of the connecting rod. The use of trigonometry

in such engineering problems is not an advantage; it is all extra work and quite unnecessary. When deductions from mathematical reasoning come to be applied in the drawing office the simpler they are in form the better. I had six pages of foolscap sent to me lately, in a set of examination papers, all to calculate the load upon each of the feet of a three-legged stool, upon which I had placed an old lady weighing one hundred pounds. A great part of a treatise on trigonometry was brought into play in the solution running the stresses up and down the inclined legs by secants, and a lot of square root and other rules. Upon the whole the work was correct, but the labour was quite unnecessary, one line, or at most three lines, was all that I wanted in the solution. The problem now before us admits of similar simplification. The effect of the inertia of the parts in vertical movement is at any instant $= \frac{N^2}{2936}$ R.W., where N is revolutions per minute, W the weight in motion, and R is the equivalent vertical radius which at N revolutions per minute would produce the same rate of change of vertical velocity. For many marine engines 54 revolutions per minute is about the average, and then $\frac{N}{2936}$ is, say, equal to 1, so that the length of R in feet is then the measure of the inertia effect of unit weight. A simple construction gives the values of R. When the length of the connecting rod is as is usual, 4 cranks, in Fig. 1, describe the crank path semi-circle K C P, and at *one foot* down describe the semi-circle L O Q with the same radius. Set off M N equal to one-fourth of the crank, and from K as centre describe the arc N A. Describe also the arc M B with radius equal to the connecting rod. The diagram is now complete. When the revolutions are not 54, the ordinates are to be multiplied by $N^2 \div 2936$: I shall describe the readings as for 54 revolutions for which no multiplying is required. The curve A D N is the datum line for the ordinates. When the crank is at F going down, the piston is F G from mid position, and the inertia effect is F H × W. The gravitation effect is however F I, and the result is that the vertical force on the crank pin is then less than the force of the steam on the piston by an amount equal to I H tons for each ton of the moving weight. When the pin is at J going down, the inertia ordinate is H J, and the gravitation ordinate is J O; these are now additive, so that the combined effect is H O × W. When the crank is at J, the piston is J G below mid-position, and the vertical force upon the crank pin is then more than the force of the steam upon the piston by an amount equal to H O tons for every ton of moving weight. The effect of the connecting rod in modifying the inertia diagram tends to reduce the inequality which is due to weight of parts, on the bearings, at the

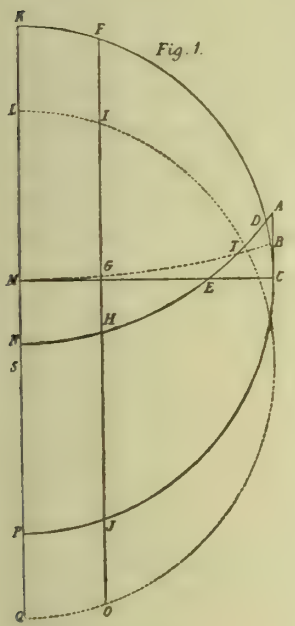


Fig. 1.

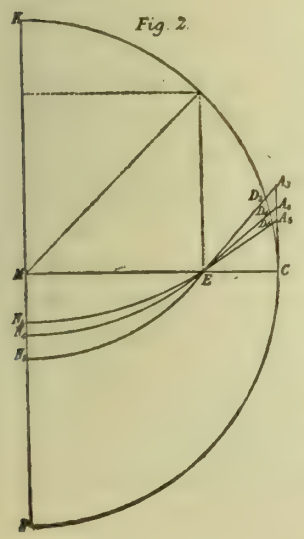


Fig. 2.

commencement of the stroke. If the diagram be for a three-foot crank, M N is three-fourths of K L, so that at the beginning of the stroke three-fourths of the difference due to weight would be cancelled by the connecting rod modification of the diagram. The varying steam effort shaking the ship is represented by the inertia ordinates. In K D N the action is lifting the hull, and in N A P it is depressing it. The simple construction given is considered to be sufficient for ordinary practice with the connecting rod equal to four cranks. A more accurate method, applicable also to other lengths of connecting rods, is given in Fig. 2. The point E is at the distance M E $= \sqrt{\frac{1}{2}} \times$ crank, found by the construction shown by the dotted square. The distances M N and C A are each the same fraction of the crank that the crank is of the length of the connecting rod. From a centre in the vertical line describe an arc through N and E, and draw a straight line through A touching that arc. For the four-crank connecting rod the centre of the curve is just one-eighth of crank below the point K. The lines shown are for connecting rods 3, 4, and 5 cranks in length. This construction is also only an approximation, but it is practically accurate. The effect of gravitation can be shown without drawing the second circle. The inertia curve has merely to be drawn again, one foot higher for 54 revolutions, or $\frac{2936}{N^2}$ for any other speed. This simplifies the diagram. The algebraic expression for R is

$$R. = x + \frac{x^2}{z} - \frac{(c^2 - r^2)\ (r^2 - x^2)}{z^3}$$

Where $x =$ height of pin above centre of shaft.
$r =$ length of crank.
$c =$ length of connecting rod.
$z = (c^2 - r^2 + x^2)^{\frac{1}{2}} =$ vertical distance between the crosshead and the crank pin.

This is not given as necessary in practice, but only to show what the approximations represent.

Mr. J. T. Milton: Sir John and Gentlemen, It will be remembered no doubt by many gentlemen here that a few years ago I had on the walls here a number of diagrams of twisting moments, so on that account this Paper is one in which I take a peculiar interest, Mr. Cole having gone over a lot of the work that I went through at that time. Now, as regards twisting moments, I dare say it will strike several gentlemen here as rather curious and rather against preconceived notions, that the ratio of maximum to mean twisting moment in what may be reasonably called a well designed two-crank engine is 1·69 to 1, and it might lead one to think that Mr. Cole has chosen a very badly balanced engine as his original engine upon which he has been trying to improve. But from my experience I do not think that this is so. I have come across several engines, as will be seen in the diagrams I have referred to, quite as bad as this which Mr. Cole has taken in regard to the ratio of maximum to mean moments, and these engines have been considered to be very well designed and well proportioned. I have not made many diagrams from triple engines, but I am not surprised to find the

ratios in these engines come out to 1·44 and 1·47. I think what strikes one as strange is, that on looking casually into the matter we look only at the indicated horse-power, and forget there is a difference in the up stroke and the down stroke which is not shown on the indicator diagrams. Necessarily the work done in the up stroke throughout the stroke is different to the work done in the down stroke owing to the weights of the moving parts, although, as Mr. Macfarlane Gray has shown, at the commencement of the stroke the obliquity of the connecting rod about balances the effect of the inertia only. There are a few points that perhaps gentlemen may think it strange for Mr. Cole to put in this Paper; he says here somewhere: "To find the initial loads, it is scarcely necessary to observe that the load is to be taken from the card, and you must measure from the top of one diagram to the bottom of the other diagram." Well, I think it is very necessary to observe that, because I must say I have seen a great many calculations made by engineering firms where they have left that point out of consideration altogether. If you are making any calculations of the strength of engines, it is essential that you should start with the knowledge of the exact load; and I find that it is a common practice to measure from one card only at a time—to measure from the steam line to the back pressure line on the same card as being the load on the piston. This leads to a great error especially in the high-pressure diagrams. It is a point that should be remembered that it does not necessarily follow that equal work in both engines means the most uniform twisting moment, and neither is this condition given by equal initial loads, but some mean between the two. I think shipowners that have got ships with engines in good order, and there are many such we know with boilers nearly worn out, will take a grain of comfort from Mr. Cole's Paper, and from what Mr. Seaton has said that in point of economy there is very little to choose between the two-crank engine with three cylinders and the three-crank engine. I hope that some of the manufacturing engineers here will make some remarks on the advisability and practicability of altering such engines. There is one point in such alterations, viz., the division of the loads, which may present some difficulty. As Mr. Seaton has remarked, it is difficult in practice to fix the cuts off of all the cylinders to give the beautiful distribution of load that you would like to get. I will not detain you any more, but I think we should be very much obliged to Mr. Macfarlane Gray for his diagram here, and his explanation of the means of taking the momentum of the piston into account along with the obliquity of the connecting rod. He says it is not in any elementary books. It is something new to me, and I think it is exceedingly valuable; I shall be very well pleased to work it out at my leisure, and I have no doubt I shall be as charmed with it as he has been.

Mr. G. W. Manuel: Mr. Chairman and Gentlemen, as Mr. Cole has mentioned my name in his Paper I would take the liberty of making a few remarks in connection with the subject of the Paper. Mr. Cole has shown us how triple-expansion engines can be adopted with the fittings of the existing two-crank compound engine of old steamers, in a very simple manner at a moderate cost. There are difficulties however which have already been noticed in regard to the unequal division of power on each crank, where three cylinders are used, and placed two above and connected to one crank, and one

above and connected to the other, which is almost impossible to avoid in practice, causing extra strain and unequal motion on the engines, more especially the low-pressure engine, increased by the use of the triple expansion, owing to the steam entering the third or low-pressure cylinder, at a lower pressure than in ordinary compound engines; and if the first or high-pressure cylinder is placed above the intermediate pressure cylinder, it will in most cases be found difficult to get the engine to work smoothly and regularly, having so little steam to cushion and to prevent knocking when the large low-pressure piston is passing the bottom centre. The engineer endeavours usually to rectify this by throwing the valve link back, a practice not advisable or economical. The plan of placing the high-pressure cylinder above low-pressure or after cylinder, is preferable, and diminishes the evil or loss to a slight extent. But in adopting the triple-expansion engine to a two-crank engine I think it preferable to use two high-pressure cylinders, placing one above the intermediate and one above the low pressure cylinder, of suitable dimensions; this would give you the benefit of the triple expansion, and the engines would be perfectly balanced or power equally divided on each crank, they would work regularly and smoothly over the centres, and economy would be the result. The high-pressure cylinders, pistons, and valves being of smaller dimensions when using steam of 140 lbs. pressure would be kept easier steam tight, and the engine more efficient. The engine would then become a proper Tandem engine, which in my opinion up to a certain power is a step in advance of the ordinary compound engine, as far as regularity of motion, and having one equal division of strain on each crank. One of the chief objections to the Tandem engine, is the friction caused by the use of *six* packing glands instead of two in the ordinary compound engine, when the rods were not carried through the top covers. Every practical engineer has experienced the excessive friction which can be put on piston rods, thereby reducing the speed of the engine through the careless manner of tightening up the steam-tight packing glands, especially the high pressure glands, and the excessive loss of steam by leaky glands on a long voyage. In Messrs. Holt's single Tandem engine there are four of these glands, making eight in all for two cranks; the trouble in ordinary Tandem engines is with the glands between the two cylinders, viz.: the one on the bottom of the high-pressure cylinder, and the one on the top of the low pressure, more especially when steam of 140 lbs. is used. This trouble and loss can now be got rid of by using my patent packing box, as mentioned by Mr. Cole, which is portable, and contains Marshall's or any similar floating metallic packing; this reduces the number of glands to four in a Tandem engine, and the friction to a minimum, and always constant, and it is out of the engineer's power to tighten this up at sea; the strain on the metallic packing rings is adjusted to a *known* pressure, before leaving, sufficient to keep the rod steamtight, without heating, all the voyage of 12,000 miles, and with no perceptible wear of the piston rod. Therefore on the score of friction or leakage of packing glands, there need be no difficulty in adopting the three or four cylinder Tandem engine, using either triple or quadruple expansion. I think shipowners who cannot afford the room, or extra expense in altering bulkheads to introduce the three-crank engine in old steamers, are much indebted, in these hard times, to Mr. Cole for the valuable suggestions contained in his Paper for the more economical working of these steamers.

Mr. W. Parker: I have not had the pleasure of hearing Mr. Cole's Paper read, but I take it that his object in reading this Paper was to show how possible it was to convert the ordinary type of compound engine into triple-expansion engine. I am sure every shipowner will receive this Paper with great pleasure. It is very well known, Sir John, that since the introduction of the triple-expansion engine a considerable reduction of coal consumption has been obtained; and steam-shipowners generally agree that their steamers that were built with the old compound engine are in the way of becoming obsolete, as compared with those that are fitted with the triple-expansion engine. Now I do not want for a moment to go into the details of Mr. Cole's Paper, but I should like to say this, that there exists a great diversity of opinion amongst shipowners as to whether an engine with three cranks is preferable to an engine with two cranks; and the majority of the engines of the compound type are two-crank engines. From diagrams and calculations that my colleague, Mr. Milton, and myself made some time ago, we came to the conclusion that there is not a very great difference in the twisting moments, whether the engine has two cranks or three cranks, providing the steam is properly admitted and cut off. We took out three different engines, one of them was a White Star engine, that of the S.S. "Britannic" or "Germanic," which was fitted with four cylinders and two cranks; the second was one of the Pacific Company's Steamers' engines—I think it was either the "Iberia" or the "Liguria"—with three cranks, and the third was the "City of Berlin," a vessel fitted with two cylinders only and two cranks. We carefully worked out the twisting moment of each engine. Now one would naturally expect, and it is the general impression, that the engine with three cranks would have a more uniform twisting moment than an engine having two cranks, but strange to say the "White Star" engine had the most uniform twisting moment. The next was the two-cylinder engine of the "City of Berlin;" and the one that had the least uniform twisting moment was the three-cylinder engine.

Mr. H. A. B. Cole: Sir John and Gentlemen. First, in reply to the remarks Mr. Seaton was kind enough to make, I am very glad to see such an authority as Mr. Seaton is on triple-expansion engines, agrees with me that over the low-pressure cylinder is the best position for the new additional cylinder. I am quite aware, as Mr. Seaton says, that it is very difficult to get the ideal diagrams to correspond with actual ones, and that is one of the reasons why I have used an index in getting out my curve, which is only supposed to be a guide, so to speak, a rough and tumble guide, to what will really take place, and I am very glad to see that Mr. Macfarlane Gray considers that that index is somewhere near the mark in real practice. Of course, my curve is a good deal worse than the adiabatic curve, but it does not come out like that in reality, we know. As to converting engines that are partly used up, and scarcely worth altering, of course I could not go into that question. I intended this Paper merely to show how a decently well preserved compound engine, as it was, could be made into a triple-expansion engine. Of course I am well aware, if the engine is altogether used up and not worth spending any money at all upon, it will be the cheapest thing in the end to howk it out of the ship, as a North countryman would say, and have a new engine. No doubt of that; but that is, perhaps, scarcely relevant. Then, with regard to this 450 pounds that Mr. Seaton was good enough to mention, I am

very glad to have got his opinion about it, but I did not mean it for the journals, I meant it for the crank pin; and, from a little investigation that I have made, I fancy it will be found that engines that are working sweetly enough, taking the momentum into account, in the way I have put it forward in my Paper (I see it must all be looked at again after what Mr. Macfarlane Gray tells us about the connecting rod), going into it in the way I have done, which I do not think is new, I believe it will be found that a great many engines will show as much as over 500 pounds to the square inch, I mean taken on the actual longitudinal section of the crank pins at the beginning of the stroke, and allowing for momentum. I am also glad to see Mr. Seaton does not see any objection to the two-crank engine as compared with the three-crank engine, on the score of economy of fuel *per se*. Then I beg to thank Mr. Macfarlane Gray very much for the kind way in which he has spoken about the index I have chosen. It is purely empirical, perhaps it is a little bit too bad for some, and a little bit too good for others; it corresponds with a ratio of expansion of 17, or something of that sort, to about 30 per cent. of condensation. That is another way of putting it, which no doubt Mr. Macfarlane Gray will understand a great deal better than I do. With regard to what Mr. Macfarlane Gray has told us about the connecting rod, I can only say it is quite new to me, and I shall take an opportunity of asking him to be kind enough to tell me a good deal more about it, it is no doubt very interesting and instructive. I am very glad of his having mentioned it, but I must say at this short notice I do not thoroughly understand it. I think, perhaps, it is a little bit unkind of Mr. Macfarlane Gray to tease me about the old lady with the three-legged stool, because of the sines and cosines, really Mr. Macfarlane Gray is the very last gentleman in the room I should have expected such a rub from! Then, with regard to what Mr. Milton was good enough to say, I was afraid my remark about the loads being measured from the top of one card to the bottom of the other card might be considered rather trite, only I fancied that, as probably everybody is not familiar with this point, it might, perhaps, be just worth mentioning. I am aware it is difficult to arrange the cut-offs just as you like, and in my diagram the cut-offs would appear to a practical man, especially in the intermediate cylinder, rather unduly early, but at the same time, as I have mentioned in the Paper, I chose a very high initial pressure, because I wanted to see where the pinch would come in about the initial loads and the squeezing of the oil out of the bearings at the beginning of the stroke, as I think that will be the practical difficulty in carrying the thing out. I do not think any one would be likely to adopt 195 lbs. initial pressure. When you come to take that pressure down 20 or 25 pounds it will be found that the cut offs will shift a good deal further along, and not be as early as shown in that diagram. In reply to Mr. Manuel, of course I know it is impossible to equalise the powers exactly, in fact I do not even attempt to do that, and I am quite well aware that you cannot tell exactly beforehand what is going to happen. I do not quite see that it is any easier to do this with two new cylinders than only with one, and anybody, I suppose, will admit that the fewer cylinders you have the better, however it is done. I thank Mr. Parker for the kind remarks he made on the Paper, and I am glad to hear that he corroborates what I put forth there with the utmost modesty, viz., that it does not seem, looking at the question all round in a practical way, that there is any very great difference between the ratio of the maximum to the mean twisting moment in a fairly successful three-crank engine and a two-crank

engine, altered in this way, where care and forethought have been bestowed on things. I beg to thank you very much for the way you have received my Paper.

The CHAIRMAN: I am sure the thanks of the meeting may be given by me to Mr. Cole for the interesting Paper, and the discussion which has arisen upon it. There is an old Eastern story with which we are all familiar where, not from the best of motives, old lamps were exchanged for new ones, but in this particular instance Mr. Cole is going to benefit us by making old engines as good as new, which, I think, will be an exceedingly valuable acquisition to the shipping interests of this country.

FLEXIBLE CRANK AND PROPELLER SHAFTING IN LIEU OF RIGID SHAFTING FOR MARINE PROPULSION.

By J. F. Hall, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; Admiral The Right Hon. Sir John Dalrymple Hay, Bart., K.C.B., D.C.L., F.R.S., in the Chair.]

The object of this Paper is to direct the attention of members of this Institution to an improved method of constructing the crank and screw shafting of steam vessels, so as to enable them to escape the multitude of strains and contortions that such shafting has to undergo.

In April, 1884, I had the honour of reading before this Institution a Paper on "Cast Steel as a Material for Crank Shafts, &c." That Paper related entirely to material, and was at the time, and since, submitted to considerable criticism. I may say, however, that up to the present time, I have seen no just cause to alter in the slightest degree my opinions, as set forth at that time. On the contrary, I have had my then convictions intensified beyond all expectations; and if I dare enumerate the number and condition of broken forged shafts that have, since that Paper was read, been brought under my notice I should considerably astonish a number of my hearers.

I may add that, as far as I am aware, not one of the cast steel shafts mentioned in my previous Paper, or of many others since made, has broken or failed through quality of material. I must admit, however, that I consider this due, in a certain extent, to good fortune, as there are many steamers that will break rigid shafts of any material, no matter if that material be twice as strong as anything yet made.

This brings me to the main point of the present Paper, viz., to show that both crank and propeller shafts quite as often as through defective material fail through being unduly bent or strained in an irregular line of bearings, which get out of line with each other, when unequal wear takes place, and when the hull of the vessel slightly alters its shape through the action of the sea, climate, temperature, or unequal distribution of cargo.

These evils are often intensified by the natural flexibility and springy nature of the hulls themselves, which the tendency of the age is to build too light.

To practical marine engineers there is no wonder that a shaft should fail when so bent in its bearings from any of the above causes, for it has to endure not only torsional strains, but it has to resist cross bending strains, alternately at every revolution, of a tensile and compressive nature on its outer skin, varying in intensity as the degree of flexure.

These strains, coupled with the great vibration they produce, distress and fatigue the material of which the shaft is composed till it becomes crystallized, and its molecular arrangement disorganized to such an extent that its vitality is exhausted, when its failure may be expected at any moment.

Sometimes, though rarely, the shaft in conforming to the irregularities in its line of bearings gives way at one or more of its couplings. This only happens, however, when the resistance to be bent offered by the body of the shaft is greater than the tensile resistance which the bolts in the coupling are capable of offering, through being either numerically weak or too small in section. To say the least, such an event occurring is a great source of annoyance and expense, for if the bolts be made of hard and unyielding material they are in danger of being pulled asunder.

Again, if they be made of suitable material to withstand such stresses without breaking, they will, by the continual stretching to which they will be subjected, become elongated in their shanks, thus fitting loosely through the coupling cheeses. In this latter case back lash will be set up, in consequence of which shocks will occur distressing in their effects to the whole propelling apparatus.

Also there will be a tendency to shear the bolts in two, as those who have to replace the same are able to testify by their ruffled feelings.

But more particularly liable to be fractured is the crank shaft section itself, through causes other than defective material.

No doubt, there are many members of this Institution acquainted, and some personally, with vessels that have acquired the reputation of being notorious crank shaft smashers. It is no uncommon event for some vessels to require a new shaft every one or two years, and in the majority of vessels, seven years is considered a good life for the shaft. In very few vessels is the fear of breakage in the shaft minimised to the extent that the fear of a boiler explosion is.

Alarming as this unsatisfactory state of things appears, it is really astonishing how very little until quite recently, marine engineers and shipbuilders have done to rectify the evil. In fact, although many devices and methods have from time to time been invented and suggested none of them have come into practical use, chiefly, perhaps, through their being complicated and costly, besides uncertain in their action. Thus it is that in the majority of cases shipowners have come to look upon the breakage of their crank and propeller shafts as an inevitable evil, consequent upon their failing from pure exhaustion caused by legitimate over work. This theory, however, I venture to say is inadmissible from the fact that in the majority of broken shafts fracture takes place, *not* in the fillets of the journal lengths where the most intense inseparable bending and twisting stresses should be concentrated, and therefore the point where fracture would most likely take place, but in the unlikely crank-pin or across the webs, where by rights no such great strains should exist.

This fact of itself should convince every shipowner that his shafts rarely break through pure over work, but that there is something else radically amiss in their construction. The object of this Paper is to point out what this something else is, and to show how to rectify it.

In a few words then this great evil, which is so disastrous in its effects, arises through the shaft revolving when bent or sprung into an irregular line of bearings.

This defect is one that all crank shafts with two or more throws are liable, more or less, to be troubled with.

In support of this theory everyday experience shews conclusively that no two bearings can be expected to wear equally one with the other even when all conditions are supposed to be equal, unless it be by chance.

It cannot then be expected that the bearings of the respective cranks one with another or with those of the propeller shafting should wear equally, especially when it is considered that the conditions are unfavourable to lead us to expect such a desirable end, because the shaft, in its rigidity, being incapable of opposing with equal resistance forward and aftward the efforts of the pistons tending to bend it, is unable to oppose with equal resistance throughout its length any tendency of wear in the brasses.

Therefore unequal wear in the brasses is encouraged, as the frictional work they are submitted to, in supporting the shaft, is unequally distributed between them.

Then their bearings are apt and actually do get out of line forward and aftward, or

both at once, with the tunnel bearings, through the straining of the hull, or the partial disarrangement of the foundation plate, plumber block bearings, &c.

Add these facts to the unequal wear in the brasses and you have the reason at once apparent why fracture takes place in the crank-pin or across the webs instead of in the fillets of the journals themselves, where the greatest legitimate strains should be centred.

It is obvious that when, for instance, in a two-throw shaft, as illustrated on Fig. 16 (Plate XLIII.), the bearings of the after crank have fallen (be it ever so slightly) below those of the crank on the forward side, and the tunnel bearings on the after side, the crank with the propeller shaft at that unsupported or partially unsupported point is liable to bend or actually is bent by the efforts of the piston centred in that locality.

As a consequence multiple, tensile, and compressive strains of great intensity will be localised in the crank pin and across the webs, in the after crank, where the bending effort of the piston is centred, and where, owing to its peculiar symmetry of form, the shaft naturally seeks relief when being overcome in its struggles to retain its true shape.

Again, bending stresses are localised and attempt to find vent in the after crank, when the propeller shaft is strained at an angle to the crank shaft, or, when the after crank, assisted by the propeller shafting, attempts to support the forward crank whose bearings have fallen.

As the forward crank is supported only on its after side when its bearings fail it, whilst the after crank has supports aft and forward to support it when its bearings fail it, the shaft is more easily bent in the after crank by the forward piston than it is by the after piston, as the shaft is not so well supported forward as aft.

These, then, are the under strains that produce premature exhaustion and, indefinitely shorten the lives of crank shafts.

On a single voyage across the Atlantic a shaft, ever so little out of line, will be bent and unbent over one million times. Is it remarkable that it should give way, or rather is it not wonderful that any shaft should last as long as it does ?

Of course the degree of bending to which the shaft is subjected varies continually, as the ship strains, and as the bearings wear more or less. Let alone the risk of fracture

engendered by these undue strains, they cannot be ignored without curtailing the efficiency and durability of the propelling apparatus generally.

No small extra power has to be expended in rotating the shafts when bent, and in overcoming the extra friction set up in the bearings.

Besides the loss of power and rapid deterioration of machinery resulting from these actions, heat is generated, which if not exactly dangerous, is always a source of trouble and anxiety to the engineer, especially in the crank section.

From the foregoing remarks, I think it may be taken for granted that, under the existing mode of fitting up crank and propeller shafts, neither the naval architect nor marine engineer can control all the forces tending to bend or distort them; no, not even if they line up, renew, and adjust all the bearings at the end of every voyage.

The fact that a good steel shaft of the same torsional strength as an iron one, though considerably less in diameter, lasts a much longer time than the latter, shews that the thicker a shaft is of a given strength, the greater is the liability of fracture, because the stresses produced when the shaft is bent increase in intensity as the square of the diameter. Consequently it appears to be certain that so long as we attempt to combat by brute strength the uncontrollable forces tending to fracture a shaft, so long shall we be beaten.

Hence, that which cannot be overcome must be eluded.

It is patent that a shaft, to successfully accomplish this want, must be capable of adapting itself freely and mechanically by yielding at certain points in its length to irregularities that may at one or more points occur in its line of bearings.

Some five or six years ago my firm, Messrs. William Jessop & Sons, Limited, of Sheffield, took up the manufacture of Thomas Turton's patent built-up crank shaft, as illustrated on Fig. 21 (Plate XLIII.), and have, since that time, made a considerable number of them.

The chief feature of this crank, as originally claimed by the patentee, was the facility with which the crank-pin part could be replaced, and he hoped to have been able to do it even at sea.

Although this desirable faculty has not been realised to the extent wished for, it has in its very failure developed a virtue in the shaft that was not in the first instance claimed

for it, viz., to allow itself to slightly bend, unbend, and even slightly twist in an irregular line of bearings without a tendency to break, or do more than elongate or distort the bolts passing through the webs and holding the shaft together. It will readily be understood that this very action, although preventing the shaft itself from breaking, increases the difficulties of taking the bolts out and the shaft to pieces, but the loss of this advantage is more than recompensed for by the other.

It was this extraordinary development in the working of Turton's shaft that led me to investigate the causes thereof, and subsequently to take up the flexible ball coupling patented by Mr. Verity, although the same was at the time somewhat complicated and cumbersome. However, as two heads are better than one, we have, together, since then considerably simplified and reduced the coupling to what we think is a practical success.

It was during the study of this ball coupling or universal joint, that it occurred to my mind that the proper place for at least a part of the flexibility was at the end of the crank pin in the after web. This idea was no sooner thought of than it was rapidly developed, and so admirably worked out by Mr. Verity, that I have no hesitation in now submitting it, viz., the flexible crank shaft, together with the flexible ball coupling, and how we propose to apply the same, for the consideration of the members of this Institution.

Before proceeding, I may mention that we have had one of the ball couplings, purposely thrown a quarter of an inch out of line, at work for over two years, and that it continues to work perfectly satisfactory.

We have also since Christmas had a flexible crank shaft 10 ins. diameter working in a steamer that had previously given great trouble with her crank shafting, and the same is giving every satisfaction.

Fig. 1 (Plate XLII.) shows transverse section, Fig. 2 section, Fig. 3 end elevation, and Fig. 4 plan of the flexible ball coupling, which may be either a part of each shaft to be coupled, or shrunk or keyed on to plain shafts. The end centre of each half coupling is cupped out to receive a ball which is inserted between them. On this ball, which retains the axis of each shaft end, oscillate the two shaft ends when they are revolving with any angular movement. For the purpose of transmitting the rotary motion of one shaft to the other a disc is formed on each shaft end. Upon the face of each disc are three projecting jaws, a and b, corresponding and engaging with each other. In order to keep the shaft ends in contact with

the ball, suitable bolts are passed through both discs, fitting loosely in their bolt holes, and under each nut or bolt head is placed a spring washer. Not only by this arrangement are the parts kept in close contact, but the spring washers admit of the discs simultaneously opening and closing upon each other, and necessarily of the jaws moving deeper into and out of gear with each other alternately at every revolution, when the shafts are rotating with any angular movement. In order to reduce friction to a minimum, a parallel piece made of suitable material is placed between the driving ahead faces of the jaws A on the driving shaft and the driven ahead faces of the jaws B on the driven shaft. These pieces are lipped under the jaws at the bottom or inner end to prevent them flying out whilst in motion. For the purpose of taking up the backlash and compensating for any wear that might occur on the driving ahead faces of the jaws, adjustable pieces made in wedge form are fitted between the driving astern faces of the jaws A on the driving shaft and the driven astern faces of the jaws B on the driven shaft, or between the other faces of the respective jaws if preferred, when parallel face pieces are not used between them.

Each of the adjustable and also the parallel pieces are slightly rounded on both sides, so that they may roll between the jaws in place of sliding when the jaws are accommodating themselves to any angular movement of the shafts.

Each of the wedge-pieces is secured and may be adjusted by means of an adjustable ring.

On the inner surface or circumference of this adjustable ring are formed internal cams corresponding with the number of wedge-shaped pieces. These internal cams are in close contact with the outer ends thereof, and by turning the ring round, the cams act upon such outer ends of the wedges, and adjust them between the jaws. After the adjustment has been made, the adjustable ring is retained in its position by inserting blocks of wood or other suitable material, between the before-mentioned wedge-shaped pieces and the heels or ends of the internal cams. Then after the blocks or packing have been inserted, they are covered by a metal plate, whereby they are held in position; and the whole presents a neat and compact flexible coupling, comparatively inexpensive, but certain in its action.

With this description of the flexible ball coupling, I will now proceed to that of the flexible crank shaft, or more properly speaking the accommodating web at the end of the crank-pin.

Fig. 5, Plate XLII., shows sectional plan, Fig. 6 end elevation, and Figs. 7, 8, 9 and 10 details. In this crank the pin is fitted or carried rigidly with its forward web. The pin-eye of the aftward web is bored out to receive a circular bush, such bush being made convex on its periphery, and through which the outer end part of the crank-pin is passed. The eye into which the bush is received is bored out to a suitable depth and of sufficient diameter to admit of an adjustable ring plate, which is made concave on its inner face to fit upon the outer side of the periphery of the convex bush. A corresponding concave part is formed in the other portion of the eye, which in conjunction with the before-mentioned ring-plate, forms a seating to receive the convex bush and allows it to adjust itself as required. This bush is split to allow of its being compressed upon the crank-pin by the adjustment of the moveable ring-plate, should the parts become worn or slack. The adjustment is accomplished by suitable bolts, the screw-nuts of which bolts are subsequently secured by a lock-plate.

If preferable, the adjustable ring-plate may be screwed into the eye of the web, as shown in Fig. 11.

A square or round-headed key placed through the web and secured by a split pin, fitting partly in a groove cut across the screw-thread on the periphery of the adjustable ring-plate, and partly in the threaded portion of the eye of the web, prevents the ring-plate from unscrewing.

To readjust the ring plate, should any wear take place, the key is withdrawn and the ring-plate screwed further in as required, and round to a point where one of the grooves on the ring-plate (there being several cut across its periphery) comes opposite the key way in the eye of the web, when the key may be replaced and subsequently secured by the split pin.

This arrangement would not only dispense with the retaining bolts, but would encroach less upon the metal between the two eyes of the web, as the ring-plate could then be made less in diameter.

It need hardly be stated, that in cases where the sufficiency of metal between the eyes of the web is a consideration, this latter arrangement would have the preference inasmuch as it leaves the web stronger, and if shortness of stroke is an object, this may be further done by making the web solid with the shaft, see Figs. 12 and 13, Plate XLII. In this latter case it might be advisable on the score of economy of workmanship, instead of

forming part of the spherical seating of the bush in the solid web, to permanently fit in the eye of the web a concave ring-plate, which may be secured by the same key that holds the adjustable ring plate.

The after crank in a two or three-throw shaft constructed on this principle, by any one of the above methods, is flexible, inasmuch as it will permit of the propeller shaft in rigid continuity with the after journal length of the crank shaft revolving at an angle to the crank section itself.

This flexibility, it will be easily understood from the foregoing description, is attained by allowing the after crank-pin with the convex bush freedom to oscillate in the after accommodating web, as well as being capable of a to and fro movement in the bush, so as to adapt itself to any bending and unbending or opening and closing action of the crank, which takes place alternately in every revolution, if the respective journal lengths are revolving at an angle to each other. This would be the case when the propeller shaft was thrown out of line with the crank section through the straining of the hull of the vessel, or again when the forward or after end of the crank shaft fell out of line, or when it fell bodily out of line through unequal wear having taken place in the line of bearings.

This crank will also admit of any lateral movement of the propeller shaft when wear has occurred in the thrust block, or when it has not been properly adjusted, as the crank-pin is capable of moving to and fro in the convex bush.

In a double or triple throw crank shaft a flexible after crank would freely allow of the crank and propeller shaft revolving at an angle to each other when, either the forward end of the former shaft had fallen out of line through unequal wear in its bearings, or when the latter shaft had been carried out of line aftward by the straining of the hull of the vessel. When, however, the after end of the crank shaft, or the shaft bodily falls out of line, circumstances arise which this flexible crank by itself cannot entirely obviate.

For in bending down the end of the propeller shaft to bring its axis in continuity with the axis of the crank shaft one of two evils will be experienced.

Either the crank-pin will be submitted to a bending strain in having to bend down the end of the propeller shaft, or the end of the propeller shaft will have to be held down by the after bearing cap of the crank shaft. This bending strain, if taken wholly on the crank-pin, would not, of course, be anything like as distressing as it would be in an ordinary built up, or

solid crank, but would still exist in a degree greater than is desirable, and the intensity of it would depend on what resistance the propeller shaft offered against being bent. This resistance of the propeller shaft would be the less, the farther aft was the first point where the shaft was supported in line and where the shaft would be bent from. As such shaft would not only be bent through a smaller angle, but would be more easily bent, as the forces tending to bend it would have a greater leverage, consequently a less bending strain would be put on the crank-pin and the propeller shaft itself would suffer less, inasmuch as it would not be bent so severely. (It may here be remarked that when speaking of a shaft being bent I do not always mean that it literally is bent, but that it has a tendency to bend.)

From the foregoing remarks, it is obvious that to save the propeller shaft from all undue strains, when the crank shaft falls in its bearings below the propeller shaft, and to obtain in all other respects save torsionally perfect independence and freedom of the crank shaft in its relation with the propeller shaft, it would be necessary to use a flexible ball coupling between the first and second lengths of the propeller shaft as shown at *c* and *d*, Figs. 14 and 15, and at *h* and *k*, Fig. 18, Plate XLIII.

It has been suggested that two flexible ball couplings, one replacing the flexible after crank, and used between the crank and propeller shaft, whilst the other, as before proposed, between the first and second lengths of the propeller shaft, would give all the necessary flexibility. See *l* and *m*, Fig. 16, and *s* and *t*, Fig. 20, Plate XLIII.

No doubt it would, but the fact that this arrangement would not allow of lateral movement (to the same extent) of the propeller shaft, through wear in the thrust block, &c., places it at a disadvantage with the former arrangement, which admits of such movement in the flexible after crank.

With regard to triple throw crank shafts, they may be rendered flexible not only in their relation with the propeller shaft, but in their own length by the use of two flexible after cranks and a flexible coupling between the first and second lengths of the propeller shaft, as shown at *n*, *o* and *p*, Fig. 19, Plate XLIII., or another arrangement would accomplish the same end, though with not the same efficiency, viz., by the use of a flexible middle crank and two couplings, *r*, *s* and *t*, Fig. 20.

It need hardly be said that flexible cranks can be used with advantage in a paddle-shaft as shown *u* and *v*, Fig. 17, Plate XLIII., which is an adaptation of the paddle shafting of the Royal Mail Steamer "Ireland."

Fig. 14, Plate XLIII., shows for the sake of illustration the crank and propeller shafting of the Royal Mail Steamer "City of Berlin," made completely flexible by the aid of an accommodating after web (*c*) and three flexible couplings. One (*d*) between the first and second lengths of the propeller shaft; another (*e*) between the latter and the tail end shaft; and the third (*f*) halfway between the two, to save the propeller shaft from bending by its own weight, if one or more of its bearings failed to thoroughly support it. The coupling *e* would permit of the outer end of the tail shaft falling freely when excessive wear had taken place in the stern bush.

Such an arrangement of these couplings, *d*, *e* and *f*, in combination with the after flexible crank *c*, would give complete and perfect flexibility to the whole of the shafting, in such a manner that it could adapt itself freely and mechanically to any irregularities that might occur in its line of bearings.

The fact that such has the ability to revolve freely in its bearings under any circumstances should, in itself, be a recommendation for its general adoption. For not only is a minimum of power required to rotate the shafting in its bearings, but vibration, friction, deterioration in the brasses and, above all, the danger of a shaft breaking is reduced to a minimum.

In conclusion it goes without saying that a crank and propeller shaft must be one of two things, either completely rigid or completely flexible.

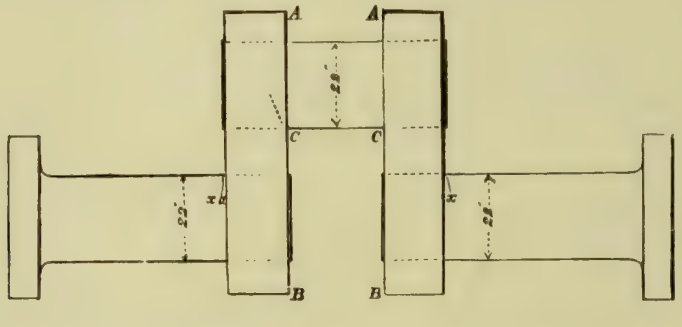
So long as half-hearted shafts are used, which are neither one thing or the other, so long will they continue to wriggle against the inevitable in the vain attempt to retain their true form at the expense of their vitality, decay and final collapse. All that was feasible and practicable without regard to cost has been done to make the rigid shaft a success in some of the finest vessels recently floated.

But the fact that they have constantly to be lined up in their bearings, is evidence that the shafts run the risk of giving way, or have actually been bent at some point or other. Hence, it is certain that rigid shafts will still continue to fail, so long as bearings wear unequally and hulls strain. Also as bearings that will not wear, and hulls that will not strain are unobtainable, the only way of consigning broken shafts to things of the past is to elude the evils by using flexible shafting.

DISCUSSION.

Mr. W. Parker: Sir John and Gentlemen, I think we shall all agree that this Paper which Mr. Hall has brought forward is one of great importance. The flexibility of shafts is a matter that engages the attention of every modern engineer, and if it could be attained in practice it would no doubt reduce the great mortality that now exists in crank shafts, of which every one connected with steamers, especially underwriters, knows to his cost. But, Mr. Hall, has, I think, taken a view with regard to the breaking of these shafts, that sea-going engineers will not endorse. His diagrams show a crank shaft and a tunnel shaft from one end of the tunnel to the other, and I take it he infers that cross-bending strains come upon each of these shafts, and that by adopting a flexible coupling, such as the one he has described, these strains will be relieved or reduced. Now, crank shafts are subjected to much greater strains than tunnel shafts, owing to the rigidity of the shaft and the liability of the bearings to get out of line from irregular wear and from the alteration in the form of the ship. These are not exactly cross-bending strains but a combination of cross-bendings and torsional strains. If one bearing is screwed down more than its neighbour, the upper part of the webs of the crank are inclined to bend inwards when the engine is on the top centre, and to again bend outwards when the engine is on the bottom centre. These strains are far more severe than were ever contemplated when the dimensions of shafts were decided upon, and if Mr. Hall's coupling could be applied to relieve these strains it certainly would be of great value. In tunnel shafts, however, the strains are purely torsional, if one bearing does wear a little more than another the shaft between the bearings is sufficiently elastic to yield. Indeed I fear from what Mr. Hall says that he does not quite understand what really goes on in steamships with regard to tunnel shafts. These shafts run on their bearings with the caps almost altogether relieved from the shafts. The caps are not so much required to keep the shaft from bending upwards, but rather as a steadiment and to keep the oil and the lubrication on the bearings; and they are frequently made hollow, merely bearing on the edges. In fact tunnel shafts are flexible within themselves, but as I said, Mr. Hall has made a good point when he refers to crank shafts. It is well known that some steamers alter their forms. We had a very good illustration of this yesterday, when my colleague, Mr. Martell, referred to the case of a steamer which when in dock showed no perceptible sign of straining, but when at sea showed decided signs of straining, even to the extent of leakage. I think that is sufficient to show that alterations of form do take place in steamers, and when these alterations are in the vicinity of the engine room a flexible joint would be of great service if it could be conveniently attached and practically applied. The Engineer Surveyors of Lloyd's Register have given the question of the breakage of crank shafts much attention and have endeavoured to devise some means for easily determining whether the shafts keep in truth while running; and I may here say that our surveyors include both men who have made shafts and those who have made many voyages across the Atlantic, and as chief engineers of our largest steamers have been responsible for the working of large shafts. Well, after carefully considering the matter we came to the conclusion that if a gauge could be made and applied to the shaft when screwed down in its working position, it could easily be detected whether one bearing was worn down more than another and whether the shaft

was running true. We had gauges of this description made and fitted in a large number of steamers, and I think I am safe in saying that the use of these gauges has had an appreciable influence in the reduction of the mortality of crank shafts. In illustration of the way in which the strains on shafts are produced I will refer to a typical case. It was the case of the "City of Berlin" some two years ago. That vessel was drifting about the Atlantic with 800 or 900 passengers on board, having broken her crank shaft. When the crank shaft, which was made of steel, was returned to this country, and examined, it was discovered that at the forward end of the shaft there was a circumferential flaw round the body, which I have endeavoured to describe here in this diagram. Now it appears that on her



outward voyage, before she left Liverpool, those in charge of the machinery found a slight flaw (x) which was marked. It was considered not to be of very great importance. But on her passage out the shaft broke at this flaw, and the vessel was towed into New York. The Americans fitted a new wrought iron end to the shaft, and that brought the vessel back to Liverpool. The shaft was then returned to the makers for the purpose of putting in a new steel end; but when they cleaned the other end of the shaft they found an exactly similar flaw (xa) in exactly the same position on the other side of the crank. Now, when you consider the nature of the strains that I have endeavoured to describe, you will see that they are sufficient to account for a great deal. If the bottom of the vessel alters form a little at one point and throws the bearing a little out of line, when the crank comes to the top centre, the two points AA would be inclined to come closer together than the two points BB, and when the crank again reaches the bottom centre, these two points AA, would be inclined to open; the consequence being that enormous strain would be thrown upon the points AA, BB, and also CC. It was suggested to the makers of the shaft, that there might be a corresponding flaw at the points C, that is to say, on the opposite side in the crank pin, and when the pin was bored out it was found that a flaw corresponding with these two flaws was perfectly visible all round the pin, proving that it was not torsional strains that had broken the shaft, but the combined strain I have endeavoured to describe. Now, within the last week or two, we have heard a great deal about broken shafts. The "Britannic" has been towed into Queenstown with a broken shaft. (Mr. Horsburgh: No, no.) Am I wrong as to the name?

Mr. Horsburgh: You are not wrong as to the name, but you are wrong as to her being towed in. She sailed in.

Mr. Parker: At all events the shaft was broken, it was broken in a different way through the web. I see here representatives of the makers of the shaft, and of the builders and owners of the vessel, and I hope those gentlemen will enlighten the members of this Institution as to why that shaft gave way. Since then the "City of Berlin" has broken another shaft, but on this occasion the breakage, I am informed, was in the pin. Mr. Hall has given us what he considers a very good means of reducing the strains on shafts, and I am sure that every one will agree with me, that if an appliance such as he has indicated can be employed without practical difficulty, such as backlash, &c., in working, it will be a very great boon to steamship owners and others. Mr. Martell has asked me to describe the instrument which I hold in my hand. It is intended to measure with great exactness whether the webs of the cranks when in an upright position are parallel or not, with a view to ascertaining whether one bearing is strained and lower than its neighbour. It is made by a Mr. Laidler of London, and measures correctly to $\frac{1}{1000}$th part of an inch. It is so designed that you can place two points at the top part of the web of the shaft and measure the distance exactly, and do the same on the bottom part of the webs. Now, having ascertained the respective distances, if we find that there is any discrepancy between them, then we know at once that the shaft is out of line and it should be lifted.

The Chairman: Gentlemen, the names of one or two gentlemen who are about to take part in this discussion have been submitted to me, but in consequence of the remarks which have fallen from Mr. Parker, I think it will only be fair to ask any of the gentlemen here who are representatives of the White Star line if they will kindly offer us some observations upon some of the points which have been alluded to by Mr. Parker, which I am quite sure the meeting will be glad to hear, and which perhaps they might desire to express an opinion upon. I think there are three gentlemen here who might be invited to do so if they are inclined. I think I might ask Mr. Pratton, the builder of the ship.

Mr. Pratton: Mr. Chairman, I may say I have not seen the broken shaft, and therefore I cannot express any opinion upon it at all.

The Chairman: Then I will call upon Mr. Horsburgh, who, I think, is superintendent of the White Star Line.

Mr. Horsburgh: I think if Mr. Reynolds were here he would be able to give us a better explanation.

The Secretary: Mr. Reynolds has left the room.

The Chairman: The name of Mr. Sennett has also been handed up to me. I have not the pleasure of knowing Mr. Sennett. If he is in the room, perhaps he will favour us with some explanation. If he is not I must invite Mr. Seaton to continue the discussion.

Mr. A. E. Seaton: Sir John Hay, I think perhaps I may apologise for the absence of Mr. Reynolds. He has not commissioned me to do so, but he has not been in the room this evening. He has had to go home, I believe, in consequence of suffering from a severe shock of headache, and left early in the

afternoon. I doubt very much, though, whether even if he were here he would not decline to enter into this controversy with Mr. Parker, on the same grounds that Mr. Pratton and Mr. Horsburgh have done. I should not care to do so myself. To come back to the subject of the breaking of cranks and other shafting, I may say that we experience every day some of the singularities of parts of machinery and physical objects generally, and we find that they do not always behave in the way we should expect them to do. My own experience shows that the shafts which ought to break do not break, and that those which ought not to break have a knack of breaking. I have now in my mind two particular cases: one that of a large Atlantic boat, in which an exceedingly heavy shaft was fitted—it is many years ago now, but it was published as an example of the strongest shaft, so far as diameter went, afloat; the other is that of a small shaft as judged by the Board of Trade Rules. The small shaft had a flaw in the after-crank pin, which had existed to my knowledge a considerable time before Lloyd's surveyor found it out. Now, this particular shaft worked for many years notwithstanding the flaw, and its small diameter, and when under pressure of Lloyd's surveyor a new shaft was fitted as large as the bearings would admit, it was found in some eighteen months or two years after that this shaft shewed signs of giving way. It was taken out, and the old shaft with a flaw was put back until a new one could be obtained, and it is running I believe to this day. Now, in the case of the other ship with the very heavy crank shaft. In her very short life she had three sets of crank shafts. In the case of the shaft with the flaw in it, the vessel had an exceedingly heavy foundation plate. She was a very strong ship, and of not very extreme proportions, but very strongly built. The very strong bed plate was no doubt the chief cause of the life of the shaft being so long. The other ship was a notoriously weak ship, and a ship that strained exceedingly in a heavy sea-way, and also her bed plate was not so heavy as it might have been.

Now Mr. Hall has come before us to-night with a very elaborate Paper, and one which must have taken him considerable trouble to get out. Not being a marine engineer he has had to read the subject up as barristers do and seek information right and left. I am sure, therefore, it is very creditable to him that he is able to come here and teach us so much as he has done, and whether we agree with him or whether we do not, our thanks I am sure are due to him for the Paper. Now, in my opinion the straining of shafts, and eventually their breaking, not theoretically, but practically is due to more than one cause. Of course every iron ship in a sea-way loaded and straining has a certain amount of general flexure. In most ships that flexure instead of being uniform, is changeable, and the change is sudden at the after engine-room bulk-head. The ship is very materially stiffened and strengthened by the very elaborate and strong engine-bed, so that probably from the forward engine-room bulk-head to the after engine bulk-head the flexure is very slight if anything at all; then comes the change, and in some particular ships it is a very sudden change in structural strength of bottom abaft the bulk head; the consequence is when a ship is bending and twisting at sea, no doubt there is more straining in that immediate neighbourhood than in any other part of the ship. This is especially true of steel ships, in which full advantage has been taken of the reduction of scantlings; this together with the natural elasticity of the steel frames allows considerable movement without doing very much damage to the hull. Then there is to my mind another strong cause of the straining of crank shafts, and that

is the weakness of the foundation plate. Perhaps I am using the word weakness wrongly, I mean want of stiffness. The foundation plate itself will not break, but it will give under the strains of the piston, transmitted to the shaft. Now, this ought not to be. If you want a shaft to work well and have a long life, it must run in a very stiff foundation plate, and not only that, but in very strong and rigid bearings. Now, it is not generally understood, and I venture to call the attention of this Institution to it, that you get very different strains on the aftermost bearing of a marine engine, to what you do on any of the others. On the forward journals the principal strain of course is in the direction of the axis of the cylinder, that is to say, with a vertical engine the principal strain on the brasses is at the top and bottom. The same applies, to a modified extent, to the forward bearing of the after crank. But when you come to the aftermost crank itself you get a continuous although not uniform pressure round the whole circumference of the bearing, and the general practice of cutting away the brasses sideways on the after bearing is therefore a very objectionable one, because you not only get the up and down strains due to its own particular piston, but you get a tangential force on the crank, and the reaction on the shaft due to the forward engine. If you do not provide for that no doubt you will submit the shaft to strains it is not calculated to bear. Now, there is one point which Mr. Hall has overlooked, but it should not be overlooked in dealing with this question. It is no uncommon thing for a new shaft not to be quite true when put into the ship. I know it is the case with crank shafts supplied by even some of the best makers in the country, that when tested before being put into a ship to find they are slightly out of truth. I think many people will agree with me in that. I will not go into the causes of this. It may not be the fault of the manufacturers; it may be sometimes due to strains received in the transit to the ship. However that may be, it is a notorious fact that many shafts are untrue before they are put into the ship, and another thing is that many of the journals are not perfectly round. Now, Mr. Hall has rather repudiated the idea that a shaft can fail from fatigue or by continuous vibration, but I do not agree with him in that. I am quite sure with marine shafts, as with locomotive shafts and axles, there is a certain period in which the shaft can be trusted to work satisfactorily, but the time comes at last when from vibration and from internal working, it gets into such a state as to be untrustworthy. That has been recognised I know by many superintendent engineers of great experience and mature thought, and generally after a shaft has run so many thousands of miles they begin to look very carefully, and more especially in the fillets, for signs of cracks. I know some engineers who take a shaft out after it has run so many thousands of miles, whether it shows signs of fracture or not, and in fact one of them, in speaking to me on the subject used the term, "I like to give my shaft a rest." Now, the plan adopted by Mr. Hall, or proposed by Mr. Hall, is to say the least ingenious. I will deal first with the couplings in the tunnel. It is very ingenious, but I do not like to say that in practice it will be found so highly satisfactory to all concerned as Mr. Hall would like us to believe; for this reason, that in the first place if the shafts have a tendency to get out of line at the couplings, and there is an in and out movement in this coupling, you will get noises that will be far from pleasant, and I do not think it is satisfactory to hear issuing from the tunnel door noises that are likely to prevent one from paying proper attention to the ordinary sounds so familiar in the engine room. Then again, supposing the noises do not exist, supposing that owing to proper lubrication there is not this squeaking which

was so common with the open coupling and driving bolts, still I think that some of the evils of the driving bolt system will be experienced in this plan. Then again I doubt very much whether the attention will be given to the proper adjustment of these keys or wedges that have been introduced to avoid backlash; and if this is not attended to there will be of course back lash, and backlash once setting in will continue and increase, so that the strains when the propeller is racing might be very severe from that cause alone. Then again I think there would be danger in the coupling from stern thrust. No doubt Mr. Hall may have allowed for some special thrust block in the extreme end of the tunnel to provide for this, but if the thrust of the propeller is to be transmitted to the ordinary thrust block next the engine bulk-head, there will be great danger of some of this gearing getting adrift, and causing trouble, so that on again going ahead they would meet with a mishap. That of course might be got over, but still there is I think a danger of it. Then again there is the difficulty which would be experienced in getting a shaft out. Anybody who has had anything to do with the repair of ships, the examining of propellers, and the withdrawing of shafts, knows the difficulty which is often experienced in getting a shaft out of place; with this particular form of coupling it would be more difficult than ever, because after the bolts had been got out the shaft would have to be withdrawn from the clutch. We know that in steam winches, that is often a difficult process: what it would be in the case of a big shaft I do not know, but it would entail a considerable amount of labour and trouble. Then there is finally the question of expense. Of course I agree with Mr. Hall that it is better to spend a little money at the outset, and have things which will prevent the breakage of shafts, than have the shafts broken afterwards, but we know that now-a-days the tendency is to spend no more money than can be helped in any part of a ship or engine. The care of course would be to have this provision or to make the ships stiff enough to withstand the strains on them, so that there should not be the getting out of line to the dangerous extent indicated. I may add that there is no novelty in making the shafts so to take the bending of the ship, because so far back as 1860, or probably before that, it was a common practice in the French navy to fit in the tunnel an universal joint to provide for the giving in a sea way of the old wooden frigates. There are also other methods which I need not touch on now, but I dare say other speakers will mention them. In conclusion I again say, notwithstanding all this, that I think Mr. Hall is to be thanked for the trouble he has taken in getting up this Paper.

Mr. B. Martell: Sir John Hay and Gentlemen, up to the present time the engineers have had all to say on this question, and I find that Mr. Hall's Paper is endorsed by Mr. Seaton in respect to this subject to which I am going to call particular notice. Mr. Hall calls particular attention to the tendency of the age to build light ships, and he attributes the accidents that occur to these screw shafts—Mr. Seaton endorses that to a great extent—to the lightness and to alteration of form. Now I entirely dissent from that. I think the tendency of the age, as far as I know anything about it, is to build strong ships, and within the last ten years I have no hesitation in saying that in all the alterations that have been made in the ships that have been built generally throughout the country, the tendency has been to increase their strength instead of the tendency being to diminish the scantlings, and make them lighter. With regard to the alteration of form producing these accidents,

I do not believe there is anything in it, and there is no evidence that has ever been brought forward whatever to show that accidents which have happened with regard to screw shafts have been due to the alteration of the form of the ship. Do you suppose that, as surveyors of ships, in going down and looking at these ships, if there was any alteration of form we should not be able to discern it. We do not even see where the shaft tunnel is cut, that round about the tunnel the caulking is even disturbed. Do you suppose that the alteration of the form of a ship would occur without the caulking being disturbed, or that the surveyor would not be able to see some symptoms in the after part of the ship to shew that there had been an alteration of form? It is all very well for engineers to come back to the naval architects and complain that it is in consequence of their bad workmanship, and because they have failed to put their shafts in true form, to say that it is not on account of that, but it is due to the alteration in the form of the ship. There is nothing of the kind. It is all absurd. There has recently been a good deal said about steel ships being springy. You would imagine that they were like the sea-serpent, and that they were continually altering in form in passing over the waves, and that consequently the bearings of the shafts must be altered. But we know of ships built to the largest scantlings, large ships, ocean going ships, Atlantic ships that have been engined by first-class engineers in the Clyde, that have gone away from there on their first voyage, without so much as a trial, or scarcely a trial of their engines, and have crossed the Atlantic and gone backwards and forwards, and have been performing their voyages now for two or three years, and there has been no complaint of the alteration of the shafting or of any heating of the bearings. Why? Because the engineers were first-class men and did their work in a first-class manner, and did not fall back on the naval architect and say, "Oh, your ship is too light; and it is the alteration of form that has done this." It is no such thing. I do not believe there is any truth whatever in the assertion, and no evidence has ever been brought forward to show that that is the cause of the failures that have happened through the breakage of screw shafting. Now, I wanted particularly to bring that before the audience, because it has been so frequently put forward by engineers in defending themselves from proper and legitimate charges of failing to make good workmanship in the first place in fitting their shafting. There is no doubt that the thanks of the meeting are due to Mr. Hall, and I particularly wish to express my thanks to him, because when he speaks of light ships, there is no doubt he refers to ships that are not built to the Rules and classed in Lloyd's Register, because if the ships were built according to their rules and classed with them, I am sure they would be sufficiently strong to prevent this alteration in form that he talks about, and to prevent these accidents from occurring, and I am supported in that by my knowledge of the only ship he has mentioned. I can only hope that, as a consequence of the remarks he makes here, if there is any fault in any way due to the hulls of ships in producing these accidents, that the owners of these light ships will be induced to come to Lloyd's Registry and build to their scantlings, so that we may prevent these accidents.

Mr. J. F. Hall: Sir John Hay and Gentlemen, in replying upon this Paper, I will take the speakers in order, if you please. First of all with regard to what Mr. Parker has said about the flexible coupling and the fact that the propeller shaft itself is flexible; of course it is flexible, I know that perfectly well, but it is flexible at the expense of the material itself. This cane is flexible (*taking*

one up for illustration) but if you are always working and twisting it about it will much sooner break than if you put a flexible coupling in the middle of it. We know from the length of the shafting it must be flexible from its own weight, but it is far preferable to let it accommodate itself on a ball joint than that it should be flexible at the expense of the very material it is made of, and I think Mr. Parker will agree with me. (*Mr. Parker here gave a sign of assent.*) I am much pleased and flattered that Mr. Parker considers the flexible crank shaft a good thing, and I am infinitely obliged to him for the very conclusive manner in which he has demonstrated to the meeting the way in which cranks break and which, at any rate proves my theory to be a correct one. I very much regret that my esteemed friend, Mr. Reynolds, is not present. I think that, although we differed very considerably in opinion two years ago, on another subject, we might have been found to be a little more in conformity this time. With regard to what Mr. Seaton has said, of course in a very strong ship with a very heavy bed-plate, I do not suppose the cranks do fail, but this idea is not for such a ship, and all ships are not so strong or have such solid heavy bed-plates that Mr. Seaton talks of, or else how is it that so many cranks keep breaking. He also says there is one feature I seem to have forgotten, namely, that some crank shafts when delivered from the makers are themselves out of truth. Well, they are always likely to be when they are paid for at such low rates as they are to-day. The wonder is that the makers can afford to machine them at all at such prices. I can assure Mr. Seaton there are no noises with this coupling as there is no sliding or working of the adjustable wedge pieces, which are so rounded as to roll when worked out of line, and the very way in which the work is taken up by the spring washers prevents any noise whatever. I have had one of these couplings at work for a very long time, and I have never heard any noise at all. It is working a quarter of an inch out of truth now on purpose. Then he took exception to keeping these keys in place with the adjustable ring. Of course there must be a little care, but the thing is so simple that, I take it, very ordinary care will suffice to adjust these pieces, and certainly not as much as is ordinarily required to line up the bearings. That great care has to be taken on board steamers in lining up bearings I do know, for when I brought this coupling and crank shaft before the attention of one of the most eminent engineers in this country he gave me this answer: "It is perfectly right, but I would not adopt it under any circumstances. If my engineers got to know that they had a flexible crank shaft they would never line up their bearings, and we should have nothing but negligence amongst them." I considered this the most extraordinary view to take of the matter I ever came across. (Laughter.) Then, as regards what Mr. Seaton said as to the difficulty of taking the propeller shaft to pieces with these couplings, I would not make these couplings necessarily to take the place of the ordinary couplings, but would make them additional, so that the shaft could still be taken to pieces by means of these ordinary couplings. You will notice that in the proposed flexible points in the illustration of the "City of Berlin's" shafting they are not shown to replace the ordinary couplings, but as additional ones. (Applause.) As regards the French universal joint that Mr. Seaton has referred to, of course I have heard about it, and I think he knows as well as I do that it is a very different thing from this. Now, with regard to what Mr. Martell has said, I must say that I am completely dumbfounded. If there is one man in this room who ought to and does know more than another that ships do warp and strain, it is Mr. Martell, and here in his very paper that he read yesterday, on page 61, I find him saying this:

"It must be evident that in the case of two similar vessels in all respects, the one built of iron and the other of steel, if they were both subjected to the same strains, sufficient in the case of the iron vessel to break her asunder, that the steel vessel, though constructed of a stronger and more ductile material must, if the strain be continued, either break or change her form."

Mr. MARTELL: Allow me to explain that in that remark I referred to vessels stranding, which is a very different thing from vessels floating on the ocean.

Mr. HALL: Very well, I will take Mr. Martell's remark on that, but on the next page, page 62, he talks about vessels labouring and straining in a heavy sea.

Mr. MARTELL: I would further explain that all ships labour and strain in heavy seas, but they do not necessarily alter their form.

Mr. HALL: Gentlemen, I am really sorry Mr. Martell is putting his foot into it so, but on the fourth line of the same paragraph he says that "where a vessel has been severely strained to such an extent as to produce distortion of form."

Mr. MARTELL: I really must in self-defence explain there that I was reasoning entirely on vessels that were stranded; most certainly, if you read a part you must read the context altogether to see the meaning of it. I referred to vessels stranding, not to vessels prosecuting their ordinary voyages on the ocean. Mr. Hall has quite misinterpreted my meaning.

Mr. HALL: I should be very sorry indeed to misinterpret Mr. Martell, but when he speaks first about "labouring and straining in heavy seas," and then he goes on to say: "This vessel had to be strengthened at a cost of more than £10,000 before she was again sent to sea———"

Mr. MARTELL: But that was a vessel not classed at Lloyd's.

Mr. HALL: Well, gentlemen, after that remark, of course, I cannot say anything further; but I am glad that Mr. Martell, after speaking as he has done, does admit that vessels that are not classed at Lloyd's may be flexible. The object of my Paper is to show that some vessels are flexible, and that therefore if you do not want to break your shafts, if you have flexible ships you must have flexible shafts. I have a very high regard for Mr. Martell, and am glad to find we agree at last. Of course, I come before you in a rather peculiar manner. I am only a poor steel-maker, and in coming here and reading a Paper on one of the most vital parts of marine engineering amongst the cleverest men in that profession in the world, I naturally feel a good deal of hesitation in what I put before you, but I must, in justice to my friend Mr. Verity, say that he has had quite as much to do with this Paper as I have had. In fact he gave me the idea in the first instance with his flexible coupling, and I am sure we are both of us very much obliged to you for the very kind manner in which you have listened to this Paper, and the remarks which have been passed upon it. I thank you all.

The CHAIRMAN: I think I may express the thanks of the meeting to Mr. Hall for his interesting Paper, and for the excellent discussion which it has originated. I know as a sailor I should feel very grateful to him if he could preventhot bearings and distortions of engines, from whatever cause arising.

ON A NEW SYSTEM OF STEERING GEAR, AND RUDDER STRAINS RECORDED BY IT.

By Arthur J. Maginnis, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 16th, 1886; Admiral The Right Hon. Sir John C. D. Hay, Bart., K.C.B., D.C.L., F.R.S., in the Chair.]

Since the first introduction of Mr. MacFarlane Gray's useful invention of the steam steering gear, numerous types have been introduced differing in application and valve arrangement, but all more or less depending upon his principle of controlling gear, which must be considered the essential point of the application of steam power to steering.

With but few exceptions, the method was adopted by all of applying the gear to the rudder quadrant by means of wheelchains (owing, no doubt, to Mr. Gray's success), and is still generally adhered to, notwithstanding the serious drawbacks of holding the rudder absolutely rigid, excessive noise, and heavy cost for maintenance both of chains and machinery.* During prosperous times, when shipping investments proved remunerative, and expenditure was not severely criticised, these points did not receive the serious consideration now given them, and it was with a view of remedying these defects that the system of steering gear now brought forward was designed, and also to still further provide against the loss or disablement of rudders, a too frequent occurrence.

As may be seen on reference to Plate XLIV., this system (which has been thoroughly tested in the North Atlantic trade) consists in placing an ordinary steering engine with the simplest possible controlling gear, on one side of the fore and aft centre line of the ship, and applying the power to the rudder, by inserting the shaft into a worm geared in the quadrant; two feathers then serve to turn the worm which is free to travel endways on the shaft to either side, so that the thrust may not be taken in any way by it, but upon a piston connected to the worm on the opposite side, which is placed midway in a cylinder full of liquid, so as to transfer the thrust to the vertical volute springs shown on top of the horizontal or cushion cylinder.

* Since writing the above Mr. MacFarlane Gray has called attention to the fact that his gear has not the defect of being rigid, and the writer has pleasure in confirming this.

To prevent the worm being forced back out of gear, and to guide it in a true central position when sliding from one side to the other, a roller bearing is placed just behind it, which is fitted with two long guide rollers placed one on the top centre and one on the back centre, outside of the circumference of the worm thread, the lower portion being formed to hold the lubricant; this, it will be observed, keeps the worm at all times true to position when it is working as a rack, at the time of yielding.

The essential feature, however, of the whole system, consists in what is termed the cushioning gear, which provides a movable thrust of the worm, and by so converting it into a rack ensures the flexibility so absolutely necessary for all rudders exposed to heavy weather. This movable thrust is obtained by placing the worm piston in the cushion cylinder, which is filled with liquid up to the hydraulic pistons in the vertical or regulating cylinders, and kept so by the pipe connected to the small reserve tank; then, when the worm is in exact mid-gear, the piston closes this pipe, and on the strain of moving the helm coming upon it, the liquid will be shut up by the worm piston, and the thrust be transferred to whichever of the spring-loaded pistons it is moving towards. Should the worm piston move from its position (which it will do owing to the spring yielding), the charging pipe will be opened behind it and liquid drawn in from the tank, to be again forced back on the return of the piston, thus ensuring the cushion cylinder being always kept full, and the certainty of the gear and rudder returning to their original position after the effects of the blow have been spent.

The strain from the volute springs is then taken upon the main crossheads, which are attached to the standards screwed into the regulating cylinders, and so arranged as to be adjustable to suit the height required by the volute springs. These latter are placed between the hydraulic pistons and tension crossheads, and can be screwed down by the tension rods and nuts, so as not to compress or yield until the load required to hold the helm hard over at full speed has been exceeded; to prevent too much compression or yield of the volutes (which might allow of the worm being forced so far over as to get out of gear) check nuts are placed upon the tension rods, which only allow the springs to yield until they come up against the crosshead.

To ensure elasticity at all times, a light spring is placed between the main and tension crossheads, which compresses or expands on the least change from port to starboard. As regards the design of the pistons, those fitted in the regulating cylinders are formed of the usual hydraulic cup leathers, whilst the worm piston, owing to its revolving and taking pressure on either side, is fitted with two cup leathers, one for each side, and so arranged that they do not revolve with the piston.

It will be noticed that by taking the thrust on the piston, one of the great power-absorbing qualities of a worm, namely, thrust friction, is to a large extent eliminated by the fact of the piston revolving on liquid.

As it is necessary that the steam gear should at all times be readily disconnected, a pipe is fitted communicating with both ends of the cushion cylinder, which only requires the stopcock to be opened, and thus allow the fluid to pass from one side of the piston to the other, when the worm will instantly run out of gear.

Owing to all the strains and jerks of the rudder coming upon the fluid, it is evident that their force can be easily ascertained: first by placing an ordinary pressure gauge on each regulating cylinder, and secondly by attaching a Richards indicator to the communication pipe, and working the barrel from the rudder head. As a result of this, the writer has now the pleasure of bringing forward what he believes to be the first actual indicator diagram obtained of the strain on a rudder.

This was taken about September last (1885) in smooth water on the run down to Queenstown, from the gear fitted on the S.S. *British Prince*, a Liverpool steamer of 4,000 tons, trading to Philadelphia.

In the December following a second set of diagrams was taken, a short notice of which, as well as of the first one, will prove interesting. By the first card it will be seen that the *power* required to hold the helm hard over was 3·5 tons for a speed of $12\frac{1}{4}$ knots and an immersed rudder area of 101 square feet; whereas on the November voyage, for the same speed, but immersed area of 104 square feet, the load as shown by the gauge (no diagrams being taken) was about 4·5 tons. With the same immersed area, but increased speed of ship to 13 knots, this load was raised to 5·6 tons, as measured on the December diagrams. A close examination of the starboard figure shows that on reaching the angle of 24 degrees, something interfered with the working of the indicator, as it failed to record the correct pressure, but fortunately an effectual check of the load remained in the pressure gauge, which, by recording 290 lbs. per square inch, proved the load for the starboard helm to be exactly the same as the port, namely, 5·6 tons.

The differences in the indicated pressure between the port and starboard figures at the angles up to 4 degrees, distinctly show the effect of the right-handed propeller, as do also the pressure gauges, the one for starboard thrust always showing pressure when the helm is amidships.

A further development of the means taken to ascertain at all times the strains or jerks to which rudders are liable is now being made, with an arrangement of two indicators (one for port and one for starboard) continuously recording on paper, which is wound from one drum to another by means of strings to the rudder head and a simple ratchet attachment, so that it is hoped the rudder will, to a certain extent, become its own autobiographer.

By watching the pressure gauges in heavy weather, some idea of the varying nature of the strain can be formed, although with the gear no noise or sound is heard to draw attention to it; but amongst several cases may be mentioned one, noticed by the writer in a heavy sea near Holyhead. The helm was over to starboard, and the gauge registering 160 lbs., equal to a 3-ton load, when suddenly the ship was felt to be lifted by a heavy following sea; at the same moment the pressure on the gauge fell and almost instantly rose on the port thrust gauge to about 200 lbs., which meant a load of about $14\frac{1}{2}$ tons put upon the rudder and gear in a reverse direction to the legitimate strain. This change of strain was again noticed during a hurricane in mid-Atlantic when a terrific following sea was running, the pressure suddenly falling upon one gauge from 175 lbs. and rising on the other to considerably over 300 lbs., or say a load of about $24\frac{1}{2}$ tons. So far as has been yet noticed, this appears to be the greatest strain yet upon the rudder and gear, but no doubt much heavier loads will be experienced, and will in future always be recorded by the arrangement already described, which it is proposed to briefly term the "Ruddergraph."

From the description of this system it may be seen that, although the first object aimed at was the reduction of heavy cost for maintenance, other essential features were not lost sight of, but on the contrary proved valuable aids in developing the cushioning arrangement, and so providing an elastic rudder holder, seldom under pressure, and capable of being adjusted to a maximum impact load, by a Supervising Board.

In conclusion the Writer would like publicly to express his indebtedness to the Owners, and also his best thanks to the Commander and Engineers for the willing assistance at all times given him, which has enabled him to bring forward information tending to advance the still faint knowledge of "*Rudders, and their strains, steering gears and their requirements,*" minor but all-important factors of the modern war-ship, "Greyhound," or pleasure yacht.

APPENDIX.

Since the writing of the Paper the steamer on which the "Ruddergraph" was fitted has returned to port and continuous strain diagrams have been obtained for a period of two days, during which time heavy weather was experienced, but no very great strains or blows of the sea seem to have been recorded, which may be accounted for by the fact that the steamer was running with all canvas set before the gale.

As already described of the Ruddergraph (Plate XLIV.), it consists simply of two ordinary indicators, the one on the right hand being connected to the starboard helm side of the cushion cylinder, and having the pencil working upwards in the usual way, so that all strains on that side will be recorded above the zero line. The indicator on the left is connected to the port helm side, and, having the fulcrum altered, all strains are recorded downward from the zero line. The paper is wound from off the right hand drum on to the left hand one, by two strings which are connected to the quadrant one forward of the centre line and one aft, by this means the movement of the rudder pulls the paper, one string acting for port movements and the other for starboard, the rewinding of each being effected by the usual spring fitted to indicator drums, together with the ratchet which allows the reverse motion.

This little instrument having been but recently designed by the writer and only once been tested, it is evident that extensive improvements can be made in it, and any suggestions will be acceptable which would tend to simplify and improve it. The continuous diagrams recently obtained show fairly great strains, but they are rather dwarfed as to height, as the strongest indicator springs ($\frac{1}{160}$th which record up to 475 lbs. per square inch) were fitted in the hope of obtaining the heaviest blows, but unfortunately owing to the paper having run out too soon these were not noted.

The various changes from port to starboard are clearly shown on the diagrams obtained when the vessel was going into and coming out of Queenstown harbour, when the strains ranged from 3 tons on the starboard helm to $2\frac{1}{4}$ tons on the port, the highest strain of 5 tons on the starboard helm was reached some time after the vessel had got well away from the harbour when the helm was put over to 35 degrees to avoid something. The next series of diagrams show that from a little before 7.30 p.m. until after 8 a.m. the next day the helm was never once put to port, although frequently brought amidships, during this period a heavy beam sea was running and all canvas set. The average strain recorded was about $2\frac{1}{2}$ tons, showing that about 8 degrees of starboard helm was constantly carried by the strains shown on the port or bottom figures, the action of the heavy sea on the reverse side of the rudder can be clearly noted.

An interesting point also shown is the varying frequency of the rudder movements, by the consumption of paper, a spell of $7\frac{1}{2}$ hours using a length of 5 feet 6 ins., whereas from 4 to 8 a.m. but $3\frac{1}{2}$ inches were used, the movement from midships to hard over used about 2 inches, and as not

more than a 2½ ton strain was reached all the time, the rudder must have been kept on the move from that angle to midships. Owing to the failure of the paper, the maximum strains were not indicated, but some days later, in the excessively heavy gales which swept the North Atlantic in March last, the springs in both indicators were broken so that the pressure reached 500 lbs. on the square inch in the cushion cylinder, making a blow or momentary strain of about 30 tons on the rudder, which is the greatest yet put upon this system of gear.

Before concluding, apologies must be made for the rather rough form in which this data has been brought forward, but the diagrams were only received from the ship a few days ago, and the writer's time has been so fully occupied as to render it impossible to go into further detail at present.

DISCUSSION.

Mr. Macfarlane Gray: It is now about twenty years since I began to scheme in connection with the steering of steam ships. In the introduction of steam steering or in any other marked invention, it is only very little that is done at a time. It was very little that I did; I may say that all I did was to apply a screw and a nut to a particular use, both the screw and the nut revolving, but at different speeds: that was the essential all. The difference of the motions of these two actuated a valve. There had been engines before that, not for steering, in which a similar valve had been employed, and its movement was effected by a screw and a nut, but the nut did not revolve. There are many more details about the apparatus, each one taken from the stock in trade of mechanical devices which are the common property of all engineers. The pleasantest part of my reward was when Sir Nathaniel Barnaby first inspected the steam steering of the "Great Eastern," and he actually patted me on the back saying, "It is beautiful, beautiful." I do claim also that I was the first to clearly see how important steam steering was to become, when no one else believed in it as a practical necessity. I said it ought to be applied to at least every steamer of 300 feet in length, and I have lived to see this more than realized. This has been all done in some months less than 20 years, and in all that time I have not seen any improvement in steam steering until Mr. Maginnis's invention which is the subject of the Paper we have just heard. As the then Mr. Barnaby said to me I now say to Mr. Maginnis—his hydraulic abutment with self filling ends and spring loaded valves with initial compression, is "beautiful, beautiful." I hope all present have followed Mr. Maginnis in his description of that which in his invention is the essential all, as the revolving screw and revolving nut were in mine. Having said so much in praise of what he has done, Mr. Maginnis will now permit me to find fault with his statement of what he calls the defects of the steam steering engine designed by me. My friend Mr. Adamson, one of our members who has had a great deal to do with large steam steering engines, has drawn my attention to the statement in the Paper that a serious drawback to my

system is "holding the rudder absolutely rigid," and that, to his knowledge, is not true; the rudder is not rigid. When I exhibited the working model of my machine here 19 years ago, I made a point of showing that the rudder was not rigid: the tiller in the working model could be forced over either way, the engine reversing, but all the time resisting with its utmost force any deflection of the rudder by the sea. For some degrees on either side the controlling wheel at the steersman's hand does not move, and immediately the overpowering external force ceases to act, the engine automatically restores the rudder's position to what it was before displacement. Afloat, this can be observed whenever an overpowering sea strikes the rudder, the engine and gear run back, and when the external violence subsides, the rudder is restored to its former position without the knowledge of the steersman; his wheel does not move; his interference is not required then. It is the quartermaster's controlling wheel that does not move to the heaviest sea striking the rudder that has deceived many into thinking the gear rigid. If I had been in Mr. Maginnis's place and writing up his invention, I would have been careful to claim rigidity as a merit, and I would have attributed want of rigidity to the other as a defect. A rudder ought to be held rigidly in its desired place up to a certain force of resistance. Imagine a large steamer with a heavy rudder at "steady" and the ship rolling: the weight of the rudder will cause it to fall first to one side and then to the other, even neglecting the blow of the sea until its movement is arrested by a sufficient restraining force. Between the steam steering engine and the rudder, there are only the friction and inertia of the parts to check this action; the steam cannot assist until the engine moves. The steam pistons are virtually what the rudder is fastened to, and they cannot be held rigidly by the steam. In Mr. Maginnis's hydraulic cylinder the rudder is anchored to a piston held by water on each side, incompressible up to the force for which the springs on the valves are adjusted. When steam steering engines are properly constructed, a very small movement of the rudder is sufficient to steam-check this tendency to motion, but the check can be applied only by a movement of the engine. I do not think there is much in this difference between the two systems; but what there is in it, is what I would have tried to set forth as a rigidity recommendation, instead of attributing rigidity to others and claiming its opposite.

Mr. B. Martell: Sir John Hay, we are very much indebted to Mr. Maginnis for bringing forward this plan of his; it is in my opinion a great improvement in steam steering gear. All those who have had experience of steam steering gear must know, notwithstanding the great advantages derived from this mode of steering, the accidents that frequently happen from the great and sudden power which is brought to bear in turning the rudder. We all admit Mr. Macfarlane Gray's claim, I am sure, to the gratitude of every one interested in this subject for his introducing it. I believe he was the first inventor of this mode of steering, which is so advantageous, and so necessary in large ships in particular. Mr. Gray has facetiously alluded to the rigidity that I referred to with regard to ships built under Lloyds' rules. I must say that from my experience I have always thought that Mr. Gray's plan of steering, notwithstanding all its excellencies, was liable to have great *elasticity*, and that elasticity led to many accidents in its use. Now in this improvement that has been suggested by Mr. Maginnis, I think that will be very largely remedied, and I look forward to a great improvement in the steam steering apparatus now introduced. The only thing that we wanted was something to

control the steering power, so that the effect on the rudder in bringing the great strain upon the stops should be obviated, and I believe this plan that has been introduced by Mr. Maginnis will obviate that. At the same time the ingenious mode that he has of registering the power that is exerted will enable us ultimately to arrive at the amount of power really necessary to govern the rudder, and cause the action to be regulated for any angle we may think proper. This is I think an important improvement, and we are much indebted to Mr. Maginnis for this valuable Paper, and the great improvement which it suggests.

Mr. W. Parker : I have only a few words to say. Mr. Macfarlane Gray's remarks have brought to my mind a strange coincidence. He referred to fitting his first steam steering engine to the "Great Eastern." As a matter of fact, some months before he fitted this engine, Messrs. George Forrester and Company, of Liverpool, received an order to make a steering gear for this ship which was almost identical in principle to the gear of Mr. Maginnis. Mr. William Esplen, a Liverpool gentleman, also designed and fitted to some steamers trading from Liverpool to Belfast an arrangement very similar to this. It was not as ingeniously arranged as Mr. Maginnis's it is true, nor did it have a Richards's Indicator to scribe diagrams. It consisted of two hydraulic cylinders with two rams attached to a tiller on the rudder-head. There were hydraulic pipes led from these to an arrangement on the bridge, which consisted of two large rams working in two cylinders by means of a worm and a segment of a worm wheel; the water was forced from the one side to the other, so that when a gauge was applied either on one cylinder or the other, we could accurately record the strain that came upon the gear, and I had an opportunity of going across the Irish Channel on more than a dozen occasions with that gear, and I took from it records of the strains that were indicated, which I have now. These records can quite corroborate what Mr. Maginnis has told us, that at times strains of a most extraordinarily severe nature do come upon these gears in a seaway, even greater than any one would imagine. I remember in the steamer "Galvanic," and I think the "Electric," we had strains indicated in the cylinder equalling 600 lbs. to the square inch when the rudder was struck by a heavy sea, whereas under ordinary conditions, while the vessel was steaming in smooth water with the rudder only slightly inclined to keep her on her course, the strains were not more than 60 or 70 lbs. But the strange coincidence is this, Mr. Esplen sold his hydraulic steering arrangement to Messrs. George Forrester and Company, of Liverpool, with whom my friend Mr. Macfarlane Gray was then connected, and he will be able to bear me out in what I am about to say. The "Great Eastern" arrived in Liverpool to have new boilers fitted, and I remember visiting her with Mr. Esplen and Mr. Wylie, one of the members of the firm of George Forrester and Co., to take measurements for this hydraulic steering gear. We met Mr. Gray, who at once saw there were defects in the hydraulic arrangement, and he set his brains to work to make a better gear, which he did by producing the steam steering engine identified with his name, and Mr. Gray persuaded the master of the "Great Eastern," now Sir James Anderson, to adopt it. It is a coincidence worth mentioning, that the ideas and principles of hydraulic steering gear which Mr. Maginnis has laid before us to-night were the same as those which upwards of 20 years ago led to the introduction of steam steering gear, and it is due to my friend Mr. Esplen that the facts should be stated.

Mr. Macfarlane Gray: Will you allow me a word of explanation? Mr. Martell brings to my mind something I ought to have said; he gives me credit for being the first person that had done anything to bring out steam steering. An American engineer, Mr. Sickels, patented a very elaborate system of steam steering. One of his engines was fitted in the U. S. S. "Augusta." It was afterwards removed to be exhibited at our 1862 Exhibition. I cannot think that his engine was so successful as mine, or the officers of the "Augusta" would not have allowed it to be removed for that purpose. It was not afterwards replaced. I met Mr. Sickels in 1874, in New York, and we then talked over our invention: two of my engines were then being erected at the works where I met him. I thought he deserved success. I think that his system was workable, but in America there was at the time no demand for such an invention. His system was to revolve the eccentric of the ordinary valve motion by hand at the speed at which he wished the engine to work. I thought it better to have a reversing valve quite distinct from the ordinary engine valves. He was then acquainted with my engine: he still thought that his plan would have acted efficiently. I do not know that any steering engines are made otherwise than with the separate controlling-valve, and revolving-screw, and revolving-nut as in my system.

Mr. E. C. Warren: Will you allow me to go to the original subject? I wish to thank Mr. Maginnis for bringing this steam indicator before us for consideration, because I consider it of very great importance; but there have been one or two things said which call for attention. I have been very much interested in obtaining strains upon the rudders of our war ships. It is very important as you well know, Sir John, that we should know as much as possible about the strains that come upon these rudders under exceptional circumstances of heavy weather, and when performing naval evolutions. During last summer we had under trial the "Rodney," one of the "Admiral" class. At full speed she made within a decimal point of 17 knots, and with her rudder having something like an area of 200 square feet and helm over $34\frac{1}{2}^{\circ}$ she was put on the circle at full speed. The total pressure registered, I speak from memory and under correction, was something like 26 tons upon a rudder of 200 square feet, in a ship between 8,000 and 9,000 tons weight, and going at the rate of 17 knots on entering the circle. Later on we tried the "Mersey," a ship going at somewhat higher speed, but not having such a large rudder, and we found the pressure registered on her was just under 20 tons under the same conditions. I will explain how we took these pressures. In the "Mersey," we took them by Crusher gauges that were absolutely reliable. In the "Rodney," we took the absolute strain by dynamometer, so there could be no mistake. In these two cases we have absolutely reliable data as to the strain. Now, when I remember what that was, and I hear Mr. Parker say he got 600 pounds on the square inch, I want to know what this means, for on the "Rodney," that would mean 7,700 tons pressure on the rudder; this I know cannot be meant.

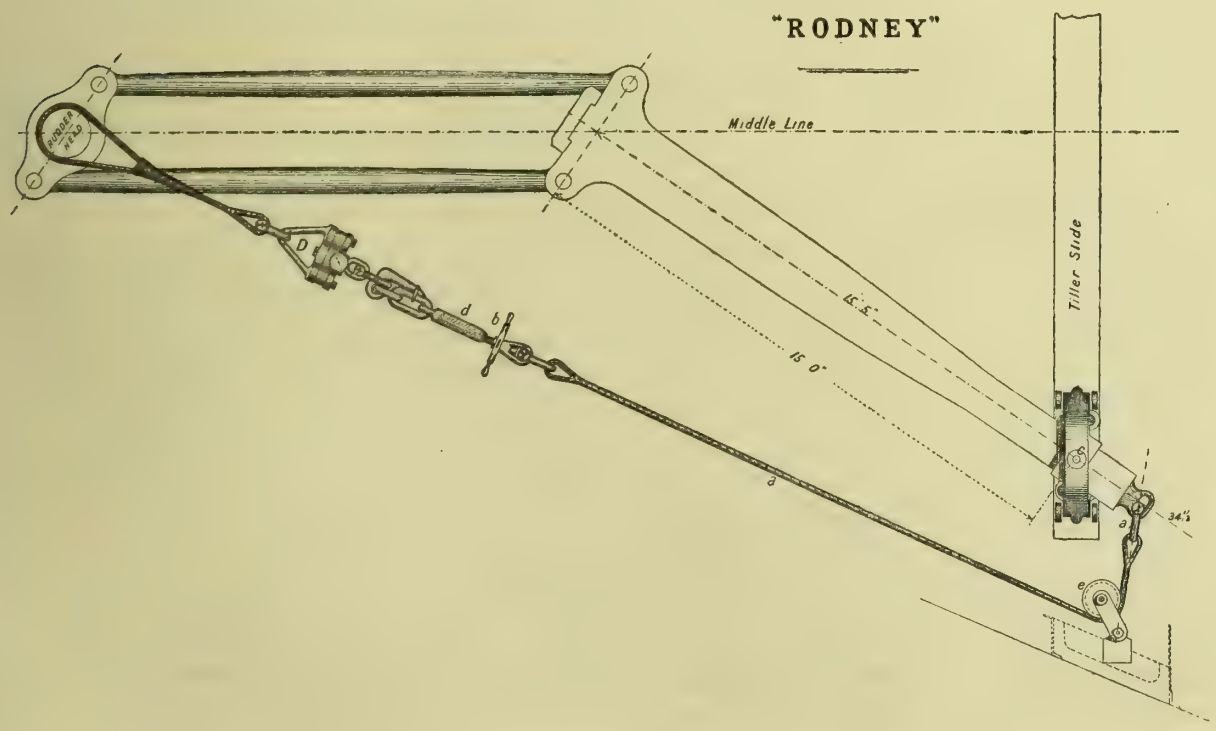
Mr. Parker: 600 pounds per square inch on the cylinder, I said, and Mr. Maginnis said the same thing.

Mr. Warren: I am glad to hear it; the object is to ascertain what strain there was on the rudder. I knew there was a mistake somewhere.

Mr. Parker: It is not a mistake. It was comparing Mr. Maginnis's figures with mine; Mr. Maginnis spoke of so many pounds pressure on the cylinder.

Mr. E. C. Warren: I will take it as a misundertanding on my part. What I am interested in and what I should like to know would be, what is the absolute strain on the rudder? Of course, the Richards' Indicator will tell us that, when we know the position of centre of pressure, length of tiller, and the immersed areas of the rudders, and this Mr. Maginnis has given us in his Paper; but, speaking of the pressures in the cylinders alone does not give us much information—it may mean anything. I give these absolute figures of two ships simply as illustrations, and if they are considered to be of any value in connection with this Paper I will give the details for insertion in the Transactions of the Institution. I wish to express my thanks to Mr. Maginnis for bringing this Indicator before us. I think, for naval officers and commanding officers of all ships, it will be a most useful thing to be able to take correct records of the strains on rudders.

Details of some experiments carried out in the "Rodney" for obtaining the rudder pressures when the ship was at full speed on the circle, in connection with the Paper read by Mr. Maginnis:



1. The sketch shows the arrangement adopted for taking the strain on tiller, and may thus be described:

a is a 5½ in. flexible steel wire rope with the standing part passed over the rudder head; D is a Duckham's Dynamometer, connected with a bottle screw, *d*, which is worked by a hand wheel, *b*; from this the steel wire rope passes round a sheave, *e*, in the ship's side, and is secured to the end of the tiller.

2. The method of taking the stress on the end of the tiller is as follows:

Put the rudder hard over by means of the steam wheels, relieve the steam steering wheels by the hand wheels, shackle the wire rope to the end of tiller, and tighten up by the wheel, *b*, then leave the handwheels free, and the wire rope has all the stress.

In this case the extreme stress recorded by the dynamometer was 9 tons.

3. To obtain from the stress on tiller end the total pressure on rudder:

Moment of tiller force 9 tons. × 15 ft. 5 in. = 9 tons. × 185 ins. = 1665 in. tons.

Area of rudder = 195 square ft.

Breadth of do. = 13 ft.

∴ Centre of pressure from axis of rudderhead

$$= \frac{5}{12} \times 13 = \frac{65}{12} \text{ ft.} = 65 \text{ ins.}$$

Let P = rudder pressure in tons,

then 65 P in. tons = moment

∴ 65 P = 1665, or P = 25·6 tons.

4. To obtain from stress on tiller end the equivalent stress on the tiller chains

The moment being = 1665 in. tons.

Let Q = pressure in tons, square to middle line of tiller, given by the block in which the tiller slides.

Then moment about centre of aux. rudder head

= Q tons × 15 ft. = 180 Q in. tons

∴ 180 Q = 1665,

$$\text{or } Q = \frac{1665}{180} = 9{\cdot}25 \text{ tons.}$$

Resolving this Q along the centre line of slide we get

Force on tiller chain = 9·25 × cos. 34½°

= 9.25 × ·8241 = 7·62 tons.

With regard to the trials on the "Mersey," where the pressures were obtained by using Crusher gauges, I regret that I cannot at the present moment furnish the details of the arrangements carried out, but I hope to do so on some future occasion.

E. C. Warren.

Mr. Arthur J. Maginnis: The first remarks that were made by Mr. Macfarlane Gray were as to the difficulty he found with shipowners of some twenty-five years ago to impress upon them the necessity of steam steering gear. I think any one who knows the shipowner of the present day will know, no matter what improvement is brought forward, it is always a difficulty to get them to accept it, if it is something out of the usual track, because shipowners trade for commercial purposes, and not for scientific, and not being inclined to run risks, the larger number of them would rather go on with what has been done than start a new venture. I may say that it was on that account that I specially expressed my indebtedness to our owners for allowing me to put this gear to work. The next point was with regard to what Mr. Martell said. I may mention one thing which fully confirms his opinion. An extremely large engine was put in with this gear, as we feared the worm would absorb the power, but we found on trial that the engine was powerful enough to put the rudder over almost instantly, if the springs were compressed sufficiently to sustain it, but, as Mr. Martell says, this gear would not allow it to do so for the reason that the springs were only screwed down to just sustain the load of the helm when hard over, whereas the quick action of the engine had put on a much greater load. This yielding quality was tested by getting the captain to put the helm hard over as quickly as possible in the forward wheel-house, and by watching the gear in the after-house, when it was noticed that the engine instantly went hard over, but the rudder did not, as the springs yielding allowed the worm to travel a certain extent to one side, and then force the rudder over at a strain due to the load the springs were able to sustain. Of course to nautical men this may appear to be a defect, but it is quite evident that we should put no greater strains on the rudder stocks than they are designed to sustain, which is the strain due to holding the helm hard over under the most severe circumstances. Then another advantage is, that when these extra heavy strains come on, the gear can be seen easing away without the slightest sound, the regulating pistons walking up until the check nuts come against the cross heads, when they can yield no further, so that if a diagram of resistance were taken it would commence with a curve up, and become a vertical line, which effectually prevents the suddenness of the blow on the rudder. With reference to the scheme mentioned by Mr. Parker, I must say I never heard of it until Mr. Parker first mentioned it to me, and then I found out that some one had been before me in applying pressure gauges to rudder strains. With reference to Mr. Warren's enquiry as to how these strains are obtained, I can explain in a very simple way. The diameter of the cylinder which takes the thrust of the worm, in this case was eight inches, and the strains were noticed on the side which has the piston-rod, so that the pressure is on the smaller area. This load being at the periphery of the quadrant, and its leverage being three times that of the distance of the centre of pressure on the rudder, therefore the strain of the 500 pounds on the square inch on that smaller piston, which equals a load of ten tons, must be multiplied by three to get the strain on the rudder. That is how I get at the pressure.

Mr. Macfarlane Gray: There is the question of rigidity.

Mr. Maginnis: In justice to Mr. Macfarlane Gray I must say that his gear was not rigid, but the fact of the matter was that the steam was too elastic, and the excess of wear and tear and noise was caused by this elasticity, that is to say, the chains of the gear are in a perpetual jig with the motion of the propeller, and the result is that after every voyage these chains have to be renewed.

Mr. Macfarlane Gray: I say the rudder is not rigid.

Mr. Maginnis: I must say that Mr. Macfarlane Gray's gear, which is not fitted with a worm, walks back when the heavy strains of the sea come upon it, and I must also, in justice to Mr. Macfarlane Gray, state that the short time of my sea experience was with one of his steering gears, and only once during a voyage of seven months were the engineers called upon to do anything to that steering gear.

The Chairman: I think we must first of all thank Mr. Maginnis for his Paper. I may be permitted to say, and I see a gallant Admiral there who will confirm me, that there is some lesson for sailors in what has been brought before us to-night. If the stroke of the following sea upon the rudder is so dangerous, some of that watchfulness with regard to using the rudder in sailing ships which we used to exercise, would, under the conditions under which steam ships may be running, be also necessary, so as to take care that the stroke of the following sea is not given on the broadside of the rudder, in a direction which would change the course of the ship and at the same time bring to bear upon it unnecessary strains from which it might be relieved by due attention of the quartermaster and captain. I think that is a point which sailors should recognise, and which Mr. Maginnis's instrument and Mr. Macfarlane Gray's former investigation bring before us in a manner which I think, as a sailor, I have a right to take notice of on this occasion. It being now considerably past the time, I move we adjourn till noon to-morrow.

RECENT MEASUREMENTS OF THE TURNING POWERS OF SCREW SHIPS.

By Captain P. H. Colomb, R.N.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 17th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

Increasing attention has of late years been given to the accurate measurement of the manœuvring powers of steam ships, and more especially to that part of them which embraces the turn of the screw ship when at speed, to the right or left of her original path under the action of the helm.

The investigation of the nature of this turn may be said to have been begun a quarter of a century ago, when the increasing length of the war ship made us familiar with the two facts that considerable space, relatively to the length of the ship, and considerable time relatively to her normal speed, must be occupied before she could make any considerable change in the direction of her path.

A great many experiments, all showing a growing precision in method, have taken place during these years in France, England, and Russia; and some distinguished names are connected with them. In England some of the earliest and best were carried out by Admiral the Right Hon. Sir Cooper Key, F.R.S. In France and Russia the names of Admiral Bourgois and Admiral Gregory Boutakov stand prominently forward in connection with the subject. In France our knowledge has been greatly advanced by the researches of MM. Risbecq, Dislère, and Lewal; while in England Sir N. Barnaby, Mr. Barnes, and Mr. Philip Watts, all of the Admiralty, have assisted to clear up many doubtful points. By far the most complete statement of the question, as it stands, will be found in the last chapter of "Naval Architecture," 2nd Edition, by Mr. W. H. White, the Director of Naval Construction at the Admiralty.

I have spoken of the growing precision which may be traced through the Methods of Measurement, and a very brief record of this may not perhaps be out of place.

Mere experience was originally sufficient to show that the path described by a screw ship in turning, approached the circular form, and that the speed was much reduced over the curved path from what it had been on the straight one. It was noticed that the speed of the engines generally fell, and often considerably, as the turn continued after the helm was put over.

The first step in measurement was to note the time it took the ship to turn completely through an arc of 360° when at a given speed, from the moment when the helm began to move. Then, neglecting reduction of speed, the time multiplied into the normal speed, gave the circumference of the "circle" of which the "diameter" taken in yards, became the space measure of the ship's turning powers.

But as it was known that owing to the reduction of speed during the turn, this circumference was exaggerated, the next step was to obtain direct space measurements.

In England, two methods long prevailed. Sir Cooper Key, then Captain Key, sought the direct measurement of the circumference of the assumed "circle" by the use of the instruments known afloat as "patent logs," and "patent leads;" of which the principle is a revolving fan with the blades set obliquely to the axis, and which being drawn through the water, is assumed to make a given number of turns in a given space, without any variation due to the velocity at which the instrument passes through the water. From the assumed circumference so measured, the "diameter" of the ship's "circle" was obtained and recorded.

Another direct space measurement was devised by Mr. Martin, master shipwright of Sheerness Dockyard, in 1865. He proposed to measure a base on the deck of the ship parallel to her middle line. At one end of this base he erected upright battens so that the line of sight when they were in transit should be at right angles to the base. At the other end of the base he placed an azimuth instrument, capable of measuring the angle contained by the base, and the line of direction of an object at a distance from the ship.

Observers being stationed at each end of the base, and the ship being set at the intended speed, the helm was put over to its full angle, and either immediately, or at some indefinitely later period, a buoy was dropped overboard, and the time noted. When the observer at the battens at one end of the base found them come into transit with the buoy, he made a signal to the observer at the other end, who noted the angle between the

direction of the buoy and the base. The result was a right angled triangle in which the base and one angle were given, and the perpendicular became the "diameter" of the assumed "circle," and the space measurement of the ship's turning powers.

Both these methods had, it is apparent, possibilities of considerable error. If the path were not circular, as assumed, the "diameters" obtained, even if the measurements were otherwise accurate, could not be relied on except within very wide limits.

But the measurement of the length of the path by the mechanical logs was liable to errors in the instruments themselves. In the second method, the perpendicular of the triangle being commonly six or seven times as long as the base, gave an inconveniently large angle opposite to it.

These two methods, however, became a real approach to accuracy, and when applied to a very large number of experiments, were capable of demonstrating several broad and important facts as to the comparative turning powers of different ships. The methods were for many years officially used for new ships, and to determine the relative values of different steering apparatuses.

In fleets, it became common to measure the assumed "diameters," by observing the angles subtended by the masts of a ship at rest, from the turning ship; and of the turning ship from the ship or boat at rest. In France, this method was largely used. But it was always found to possess the inherent defect of very small observed angles. And though all that could possibly be done with it in France had been done, yet a French writer in 1875 was able to assume, apparently without challenge, that there did not then exist any decidedly efficient method of measuring the turning powers of ships.

It is worthy of observation here, that up to the time I am speaking of, the circular character of the path described by a ship in turning, had been so generally accepted and so widely assumed, that even though it had come to be known that the early part of the path was very far from circular, there was much less effort made to investigate its nature, than that of the final part of the turn, from 180° to 360°, which was evidently more nearly circular. And in France much attention was even bestowed on the movement of the ship after she had completed several revolutions round a central point. So, neither in England nor in France, were many efforts made to measure turns of 45°, or 90°, from the initial

movement of the helm. There was not an absolute neglect of these measurements, but they did not attract anything like the attention which was given to the whole turn of 360°.

But the experiments made were disclosing two further important facts, namely, that the rate at which angular velocity was taken up in turning, varied as the turn advanced, and became uniform late in the turn. And also, that the point in the middle line at which the normal to the curve the ship was describing was also perpendicular to the middle line, was far before, and sometimes very far before, the centre of gravity of the ship. In other words, there was a sensible, and sometimes a considerable, angle between the normal to the curve the centre of gravity was describing and the perpendicular to the middle line at that point. In France this angle was called the *angle de derive*, and in England we translated it freely as "drift-angle."

Considerable light was thrown upon the question by the observation of these two facts, and at the same time their recognition stimulated ingenuity towards the attainment of greater accuracy in the measurement.

In France many plans, several of them exceedingly complex, were proposed, and some were tried. But the chief advance was made in the year 1876 by M. Risbecq. He proposed to measure a base as long as possible on the deck of the ship, and to place an observer, with an azimuth instrument, at either end of it, causing them to observe simultaneously the angles between the base and the line of direction of a buoy placed as near as might be to an assumed "centre" round which the ship was about to describe a curve approaching the circular form. The observation by a third observer, employing an Azimuth instrument and a distant point of land as the zero point, of the angular motion of the middle line of the ship, completed a system of triangulation which enabled the actual position of the ship to be fixed at the moment when the helm began to move, and at any subsequent moment during the progress of the turn. Then the curve drawn through these points, so fixed, became a graphic representation of the actual path described by any given fixed point in the middle line of the ship.

This method was first employed in England with much success by Mr. Watts of the Admiralty in measuring, in 1877, the diameter of the final circle described by H.M.S. "Thunderer." The results were published in the report on the stability of H.M.S. "Inflexible," but being carried out to verify calculations as to stability, inclination to

the vertical under definite heeling forces, and vertical position of centre of gravity, the experiments, though throwing great light on the character of the turn we are considering, did not fully develope it.

It fell to my lot to command this ship at a later period, and also to fix the form and fill up the details of the new system of measurement, in order that it might supersede the less accurate and less complete methods in use.

A trace of one of these measurements is given in Fig. 1 (Plate XLV.), and the complete method may be shortly described.

The base being measured on the deck, and azimuth observers stationed at each end of it, and at some convenient spot to observe the distant points of land; a fourth observer is stationed to note the times; a fifth, and in twin screw ships a sixth, to note the revolutions of the engines. Another observer is stationed below to watch and note the absolute movements of the tiller; while general notes are taken by an officer supervising the whole, as to the direction and force of the wind, any casual errors which may occur to mar or modify the result of any series of experiments, or any matter of importance in connection therewith.

A buoy B, with a small flag on it, and sufficiently sunk by weights to guard against wind drift, is dropped overboard. The ship, after steaming away from it, is again steered towards it on such a course as to make the observation of the distant point of land easy during every part of the approaching turn of 180°.

The ship is steered as steadily as possible, the engines being set at a given number of revolutions on a course A. D., and when she arrives in a position such that the buoy is about three ship's lengths distant, and bearing about 20° or 30° before the beam, some loud sound, such as the blast of the steam whistle, is made, and the order is given to put the helm over to the pre-arranged angle. The sound is a signal to all the observers to observe and note. The base being E F, the angles E F B and F E B, are observed and noted. The tiller is watched, and the moment it reaches the prescribed angle the time is noted. The time when the sound was made is also noted; the bearing of the point of land is fixed, and the sight vanes of the azimuth instrument moved on 45°, and set in that position. The mechanical counters in the engine-room are at the same instant read off and registered.

The ship is now turning to the right, and the central point of the middle line C^1 is describing the curve C C^1. The observers at each end of the base are keeping their sight vanes in transit with the buoy. The third observer is watching the moment when the distant object shall come into transit with the sight vanes set at 45° from the original bearing. The instant this occurs, the centre of the middle line being then at C^1, the steam whistle is again sounded, and all the observations are repeated.

The ship continuing to turn, the same process is carried out, when her head has turned 90°, 135° and 180°. That is, when the central point of the middle line C^1 has arrived at the points C^2, C^3 and C^4.

The whole of the observations being recorded in a form arranged for the purpose, the plotting is done by construction, and we get the various positions of the ship and the curve described by her centre, as in Fig. 1.

The drift angle at the centre of the ship is also taken out by construction at any of the positions where it is desired to obtain it—as, for instance, at C^3, where it is found to be the angle G C H = 11°.

It is usual, if time permits, to make three turns under the same conditions, so as to secure a mean result; but Fig. 2, which exhibits the result of a series of three such turns, is sufficient to show, first, that there is not a great deal of variation in the turn when the circumstances are the same; and, secondly, that the results of a single series of observations, when there is no reason for doubting them, may be taken as a fairly accurate measure or survey of the ship's turning powers. In the case of Fig. 2, the three small circles show the position of the centre of the ship as determined by observation when the head turned 45°, 90°, 135° and 180°, or 4, 8, 12 and 16 points of the compass, in each of the three experiments. The mean curve is drawn through the centres of gravity of the positions.

Having now these mean positions and the curve drawn through them, we next come to its analysis. At first I used the co-ordinates for the purpose of analysing the curve and comparing it with others; and then, for convenience, the ordinate was called the "advance" and the abscissa the "transfer." But I have found that for the practical purposes which have chiefly drawn my attention to the subject, it is more convenient to employ the chords C C^1, C C^2, C C^3, and C C^4, their lengths, and their angles with the original course the ship was steering, as the space measurements. The chords are called those of 4, 8, 12, and 16 points.

In the space measurements, the unit is taken as the ship's length between the perpendiculars, as this gives a standard of comparison between different ships.

The times required for a ship to turn through the different portions of the curve must be referred to some unit, in order that the results in different ships may be compared; or even that the results in the same ships with different speeds or helm angles may be compared.

There is much greater difficulty here than in the space measurements. It is easy to get the mean speed of the ship as she travels between C & C^1 or between C^1 and C^2 because the length of the path can be measured, and the time occupied in passing over that measured space is known. But there is great practical difficulty in ascertaining what the exact speed of the ship was at the moment when her centre arrived at the point C, and the helm began to move. All that has yet been found possible is, to order that the engines are set at a certain fixed number of revolutions, and to accept the speed due to these revolutions as the speed before the helm moved. But this is so far unsatisfactory that it is difficult, under present arrangements, to make absolutely sure of the number of revolutions.

On the other hand, there is natural reason to believe that some time must elapse after the helm is put over before there can be any very large reduction in speed as a consequence of helm action. The experiments, so far as they have gone, tend to confirm this view on the whole. There is, moreover, at present, no reason to suppose that different ships show very marked differences in the *proportionate* fall in their speed due to the early effect of the helm. Hence, in default of more accurate measures, it is found most convenient to take the time measure from the mean speed during the time the ship has been passing over the first four points of the turn.

It may here be remarked that the curve from the moment the helm begins to move until the ship has turned four points of the compass is called the "first octant;" the curve from four points to eight points, the "second octant," and so on.

Then the mean speed over the first octant being accepted as the normal speed, a unit is got by ascertaining how many seconds it would take the ship to pass over her own length at that speed, and this number of seconds is then called the "time-length," and becomes the unit of comparison for time, as the length of the ship becomes the unit for space.

The curve being drawn as C, C^1, C^2, C^3, C^4, the chords—called the "chords of 4, 8, 12, and 16 points"—are drawn and measured; their angles with the original course ascertained and noted, and the lengths of the chords in terms of the ship's length are set down beside each. The length of each octant is measured and converted into units, if desired.

In the case of Fig. 2 we have the chord of four points, equal to 3·19 lengths, at an angle of 10° from the original course. The chord of eight points is 5·13 lengths, at an angle of 31°. The chord of twelve points is 6·26 lengths at an angle of 52°, and the chord of sixteen points is 6·37 lengths at an angle of 72°.

These measurements are above the average, and show that the ship, if she is doing her best, is deficient in turning power. If, for instance, she were of the moderate length of 350 feet, she would require a space of 743 yards to turn in.

It is a common feature of ships with deficient turning powers that the angles of the chords should be large. There are occasional cases where large angles and good turning powers are combined, but the general rule seems to be that if the angles are large the chords are long, and vice versa. Thus, there are ships which turn better than the one in question, where the angles of the chords of 8, 12, and 16 points are about 25°, 43° and 61°; and where the corresponding lengths of chords are 4·63, 5·30, and 5·19.

The position may be stated generally, that the more nearly the whole curve approaches the circular form, so much the more deficient are the turning powers. The law is not universal, but has shown itself in the large majority of the experiments tried.

Passing now to the lengths of the octants, which to some extent govern the time required to turn in, it is to be observed that the first octant is—so far as experiments have gone—invariably the longest. Commonly the third and fourth octants are nearly equal, and are generally shorter than the second. It is the general characteristic of a ship which turns quickly in small space, that the first octant is markedly longer than the second, or any of the others.

So in time measurements, it is universal so far, that the time occupied in passing over the first octant is greater than that occupied in passing over any other. It is almost invariable that the time occupied in passing over the second octant is less than that occupied in passing over any other; while generally, as in the case of Fig. 2, the time

occupied in passing over the third octant is nearly equal to that occupied in passing over the fourth.

Concerning the fall in speed during the passage over the curve, the greatest fall is in the mean speed over the second octant, as compared with that over the first. The fall is less between the speed over the third octant as compared with that over the second; and the fall in speed over the fourth octant is commonly slight.

The fall in the number of revolutions is not generally so great as in the speed through the water. Thus in the case of Fig. 2 while on the second octant the fall in speed is 12 per cent., the fall in revolutions is only 2 per cent. On the third octant the fall in speed is 15 per cent., while the fall in revolutions per minute is only 4 per cent. On the fourth octant the fall in speed is 21 per cent., but there is no further fall in the revolutions.

But in the variations in speed and revolutions, there is nothing like the precision that is to be found in the variations in the space measurements. At present, time measures can only be spoken of in very general terms.

Fig. 2, with the elements of the turn thus analysed, exhibits the turning powers of a modern war ship at the moderate speed of about 11 knots, using the utmost helm angle of 34° and therefore at her best. She is a ship below the average in turning power, but in the present state of our knowledge, the reasons are not obvious why she should be deficient. It is, however, striking to observe in a diagram drawn to scale, how very large the space, and how very considerable is the time, which is found to be necessary for a steam ship to manœuvre in, and how very necessary some knowledge of these spaces and times becomes to the navigator in crowded waters, or approaching other ships.

I pass on now to observe that each ship appears to have a certain *character* in her curve of which she cannot rid herself, and which is the result of the particular disposition and ratios of those elements which go to hasten or retard that balance of forces which is reached when the ship's path becomes the arc of a circle.

Amongst these elements, the moments of inertia, acting adversely to rotatory movement at first, but on change to moments of momentum, acting favourably to rotation at last, are the most prominent. But, except in very general terms, and from observing the character of the curve in turning, it is difficult to speak of these elements.

In general terms, there are ships where the weights are obviously so arranged longitudinally as to show concentration about the middle point, and therefore, small moments of

inertia about the centre of gravity. There are other ships where the weights have obviously greater longitudinal distribution. The effect upon the curve of the lesser moments of inertia would probably be a more early condition of balance or equilibrium, on the curve, than when the moments of inertia were greater. But then it might be anticipated that where the moments of momentum were small, the angular velocity would be less, and where they were large, greater.

Looking at the two curves given in Fig. 5. (Plate XLV.) from this point of view, we should assume that the moments of inertia about the centre of gravity were greater in No. 2. curve, than in No. 1. And it is in fact so in the two ships, though other elements may be present to differentiate the curves.

Other elements are more easily measured than the moments of inertia, and their effects more easily estimated. The ultimate helm angle is one of these, and comparisons have been made between turns where the helm angle was the only element varied, and, therefore, the value of any given increase, or decrease of helm, could be expressed in terms of the chord and time-length of the corresponding arc.

The speed of helm movement is another element of which strict account can be, and has been taken, and its effect on the time and space measures of the turn experimentally ascertained. But in estimating the effect in different ships, or at different speeds in the same ships, I have found it convenient to employ a unit of measurement which I have taken as the number of degrees the helm would move in a time-length, that is while the ship is passing over her own length. I have found it necessary to employ a term to denote this element of proportionate speed of helm-movement and have proposed to call it the "co-efficient of tiller-power." If the time occupied in putting the helm over to any given angle was equal at all speeds of the ship, then the co-efficient would be greater as the speed fell. Steam-steering gear has greatly increased the speed of helm movement, so that when manual power would only show a co-efficient of 8 or 10, steam power would give 50 or 60.

The proportionate area of the rudder to the area of immersed middle line plane, is an element useful for comparison of the turning powers of different ships. I have found it convenient to speak of it as the "co-efficient of rudder power." It is by no means, as Mr. W. H. White shows, a satisfactory measure of rudder power when at a given helm angle; first, because it supposes no variation in the form of the rudder; secondly, because not only the area but the length of the middle line plane in proportion to its depth must be taken into account; and thirdly, because the form of the ship may in some cases offer a less resistance to rotation than in others. But still the measure is convenient and useful.

The element of length in proportion to depth of middle line plane, I have found convenient to separate under the name—not a very satisfactory one—of the "co-efficient of absolute resistance." All other things being equal, there is greater resistance to rotation when the depth of the immersed middle line plane is small in proportion to its length. Therefore, in comparing this "co-efficient of absolute resistance" with the "co-efficient of rudder power," we are able to see whether there is any real deficiency in rudder area.

The last easily ascertainable and easily expressed element is the position of the centre of gravity of the immersed middle line plane, generally called the centre of lateral resistance. Assuming that the powers of ships are alike as regards the escape of water which must be displaced when rotatory motion is given to the ship, it is evident that the position of the perpendicular to the water-surface, drawn through the centre of lateral resistance, will be one of the elements determining the point about which this rotatory motion takes place, and consequently, the moment of the rudder power. But I have found it more convenient to use a figure indicating the proportionate excess of draft by the stern for the purposes of comparison, and I use the not very satisfactory term "co-efficient of relative resistance" which is obtained by dividing the excess of draft by the stern in inches by the length of the ship in feet. This is a co-efficient which generally shows itself adverse to turning powers. Where it is large, space and time required to turn in, are generally large also.

Variations in the value of these co-efficients produce—as already noticed—differences in the form of the curve described in turning which are generally characteristic in the ships.

Fig. 3 (Plate XLV.) represents the traces of three ships' turning powers, the speeds and helm angles being nearly equal in each case, and the scale being reduced to the unit of a ship's length. No. 1 is the curve of a small unarmoured vessel of 600 tons. No. 2 and No. 3 are the curves of two different ironclad turret ships of different build, but of nearly equal tonnage, exceeding 9,000 tons.

Differences in character still more marked are seen in the traces given in Fig. 4 (Plate XLV.), where No. 1 is the turning curve of a small steam pinnace, and No. 2 that of a 9,000 ton ironclad, each being under the same conditions of speed and helm angle.

Fig. 5 exhibits the same diversity of character in the form of the curve, though the two ships are both large ironclads under like conditions of speed and helm angle.

It may be observed that in all these curves, except No. 3 of Fig. 3, the advantage in the space measures is always on the side of the curve where the length of the first octant is markedly greater than that of the second. The experiments have shown that this condition approaches the position of a law with all classes of ships; and it points towards the assumption that when the balance of the various forces at work upon a turning steam ship is reached early, the ship will require larger space to turn in than when this balance is reached late in the turn. In other words, that a considerable drift angle is one of the characteristics of a ship which turns in small space.

This balance of forces appears to be very generally reached before the third octant is completed, and often before the second octant is complete. This may be assumed when C^1, C^2, C^3, and C^4, all lie upon the arc of a circle.

Variation in speed may be expected to vary two, if not three, of the co-efficients before mentioned, and therefore to vary the curve described. An increase of speed will certainly reduce the co-efficient of tiller power, as a decrease of speed will increase it. An increase in speed may also reduce the speed of helm movement absolutely. It may also prevent the full helm angle being reached at all, in consequence of the increased pressure on the rudder. A difference in speed may also alter the trim of the ship, and therefore the co-efficient of relative resistance. In large ships, moderate increases or decreases of speed have not been found to modify the curve, but there are instances in which a variation in curve may be traced which can only be due to increased or reduced speed. In small vessels, where the change in trim has been marked, there has also been a marked difference in the curve—not so much in its character, as in its length.

As regards the general question of the powers possessed by steam ships at speed when that of the engines s not varied during the turn, a considerable number of measurements have now been made with a great variety of war ships, and they tend to show that the curve No. 1 in Fig. 3. represents very bad turning powers, and the curve No. 3 in the same figure, exceptionally good ones. That curve, in fact, stands alone, and is remarkable as being what a particular ship had shown herself able to do when turning to the right. The same ship when turning to the left, makes a much larger curve.

I have added a Table showing the analysis of each of the curves exhibited in Figs. 3, 4, and 5, which is useful in showing side by side, the space and time measurement, and the more easily ascertainable elements which have affected the nature of the curves.

Turning Powers of Different Ships Compared and Reduced to Common Measures.

Reference to Figures.	Direction and force of wind.	Speed due to revolutions ordered.	Speed over first octant.	Ultimate helm angle.	Co-efficient of tiller power.	Co-efficient of rudder power.	Co-efficient of absolute resistance.	Co-efficient of relative resistance.	1st Octant.					2nd Octant.					3rd Octant.					4th Octant.				
									Length of chord.	Angle of chor .	Time-length of arc.	Reduction per cent. on speed due to revolutions ordered.	Reduction per cent. on revolutions ordered.	Length of chord.	Angle of chord.	Time-length of arc.	Reduction of speed per cent.	Reduction of revolutions per cent.	Length of chord.	Angle of chord.	Time-length of arc.	Reduction of speed per cent.	Reduction of revolutions per cent.	Length of chord.	Angle of chord.	Time-length of arc.	Reduction of speed per cent.	Reduction of revolutions per cent.
		Kts.	Kts.	Deg.	Deg-				L'ths.	Degs.	T. L.	Kts.	No.	L'ths.	Degs.	T. L.	Kts.	No.	L'ths.	Degs.	T. L.	Kts.	No.	L'ths.	Degs.	T. L.	Kts.	No.
Fig. 3. No. 1. Ship F.	↙	10·31	8·10	32	31	·0215	·058	·168	3·56	8	4·12	21.5	—	5·83	29	2·84	+ 10·8	—	7·36	54	3·33	+ 12·1	—	7·46	78	3·43	+ 3·9	—
Fig. 3. No. 2. Ship T.	O	10·38	8·61	34	43	·0221	·094	·063	2·97	5	3·07	17·1	13·4	4·62	26	1·94	+ 12·0	+ 8·0	5·30	43	2·00	15·0	+ 8·0	5·26	63	2·35	11·0	+ 2·0
Fig. 3. No. 3. Ship N.	—	10·26	8·07	33	61	·0189	·084	·063	2·31	12	2· 4	21·4	27·9	3·43	27	1· 5	7·8	+ 19·0	4·26	46	1· 7	8·5	+ 37·6	.4·26	63	2·25	30·6	+ 15·6
Fig. 4. No. 1. Ship SP.	—	—	7.00	35	29	·0372	·076	·073	3·10	17	3·84	—	7·4	4·75	35	3·34	22·0	10·5	5·79	56	3·28	20·0	10·5	5·89	76	3·04	20·0	2·5
Fig. 4. No. 2. Ship T.	↙	7·78	6·93	34	59	·0250	·085	·168	2·90	7	2·93	11·0	+ 6·0	4·21	23	1·71	12·0	3·0	4·91	41	1·97	20·0	10·0	4·77	58	2·17	31·0	8·0
Fig. 5. No. 1. Ship E.	↖	12·0	11·22	33	53	·0243	·071	·080	3·22	13	3·29	6· 5	5·7	5·02	32	2·95	22·6	6·8	6·03	53	3·12	26·4	9·2	6·05	74	3·24	27·9	11·2
Fig. 5. No. 2. Ship D.	↖	10·9	9·03	34	43	·0207	·079	·081	3·06	9	3· 3	17·2	14·0	4·07	25	1· 9	30·8	0·0	4·55	42	2· 1	40·4	5·8	4·48	60	2·5	40.4	10·7

The reduction per cent. on speed and revolutions is calculated from speed and revolutions on 1st octant, except on 1st octant itself, when it is calculated on revolutions *ordered*, and on speed due to the same.

DISCUSSION.

Mr. W. H. White: My Lord, I should be extremely sorry that such a Paper as this should pass without discussion, but I think there are other members present far more competent than I am to speak of the facts as to turning trials. It is quite true that I have made a very careful study of the subject, and therefore I may, perhaps, be permitted to say that I did so largely at the suggestion of Sir Cooper Key. When I was lecturer at the Naval College it was a matter of peculiar interest to naval officers studying there that they should be put in possession of the facts, and so far as we know the principles, affecting the turning power of ships, and Sir Cooper Key suggested that it would be a good subject for a series of lectures. I am bound to say that in the preparation of that course of lectures I learnt more than I had known before, because I looked up all the authorities, and I was helped very largely by many eminent naval officers, including the writer of the Paper to-day. At that time what was most evident (I am speaking now about the year 1875 or 1876) was that we wanted accurate measurements for tactical purposes. The best information that I could then find was already about twelve years old, and that was the information got together by the French Experimental Squadron of 1863 to 1865, and I believe it was almost entirely due to Captain, now Admiral, Bourgois, who was the Fleet Captain to Admiral Bouet-Willaumez. His pamphlet on the trials of the squadron contained details of actual performances of ships, and methods of measurement, that were most instructive, but which so far as England went had practically been a dead letter up to that time. It was the practice then, and it had been for many years, in connection with the steam trials of the ships of the navy, to make turning trials to test the steering gear. Now, it is not so many years ago when one common method of estimating what was called the diameter of the circle was for various observers on board to make rough guesses which were added up, and the mean of the guesses was taken as the recorded diameter. My father-in-law, Mr. Martin, who was then at Sheerness, was asked to make a series of experiments with the Lumley rudder, and the facts as to the experiments will be found in the Transactions. It occurred to him that something more exact was desirable if any fair comparison was to be made between the new rudder and the old. That led to his device of the arrangement described by Captain Colomb at page 372 of his Paper, the principle of which is a very simple one, that the angle in a semicircle is a right angle. It is only right to say that Mr. Martin thoroughly understood that he was only getting what is now called the "final" diameter, and he was not accustomed to apply the method until the ship had settled down into a fairly circular path. Of course, as Captain Colomb pointed out, for tactical purposes that is not the question, and that was fully appreciated by Mr. Martin at the time. A description of Mr. Martin's method is given I think in the *Transactions* of 1865, and until this matter was taken up afresh, I believe about 1876 or 1877, no better method than Mr. Martin's was in use in the navy. There were various plans, towing patent logs, and other devises, for attempting to get the length of the path traversed, but there was nothing better established. Now clearly this was a sailor's matter, and when I began to look into the subject I said so publicly, both in those lectures that I have referred to, and in another lecture that I gave at the United Service Institution. The United Service Institution also

in one of their Prize Essay competitions gave great prominence to this matter of tactics, and so helped forward very much systematic observation and computation. I am glad to say that the Prize Essayist is here, Admiral Fremantle, and that his Paper is deserving of being read with attention as bearing on the matter, but from that time I do not think I exaggerate when I say that the officers of the Royal Navy have devoted themselves to this matter and have quite made up for lost time. Amongst those officers Captain Colomb has distinguished himself in a manner that will always redound to his credit. He was in a position to make many trials, and he has systematised the matter. He has drawn a description of his method of observation up in a practical form, and it is now the established practice of the navy to make these trials and give to the captain of every ship, in the "ship's books," a record of the results of the turning trials of the ship, and of the space she requires to turn in at different speeds, and at different helm angles, so that if he wants to perform a manœuvre, even if he succeeds some other captain, he has the information to his hands in a form ready for use. All these things have been done by naval officers for themselves, as they must be, and incidentally we as Naval Architects have learnt a good deal that is useful to us as to the effect of helm angle, under water form, and change of trim, and I can only hope that the great mass of data that is being got together, thanks to the efforts of naval officers, will help us shipbuilders to go still further in the way of improving the handiness and manœuvring power which, under many circumstances, must be the key of success in action.

Vice-Admiral the Hon. E. R. Fremantle: My Lord and Gentlemen, I must begin by stating that I consider myself quite a humble follower of the two last speakers with reference to the question of the turning of ships, and in fact that the whole knowledge which I have of that subject, or by far the greater portion of it, is derived directly from their writings. Mr. White's valuable contribution has been specially alluded to, and I may mention that these diagrams which we have here of Captain Colomb's have seen the light previously, and have been a great advantage to the officers of Her Majesty's ships, although I am not aware that they have ever been published for the information of the public. If, my Lord, I venture therefore to make a few criticisms, I trust it will not be supposed that I wish to underrate in any degree, even the slightest, the exertions of Captain Colomb. Now, I would venture to think that this new system of measurement and definition of his, although perhaps more scientific, is scarcely as intelligible to the ordinary mariner as his own original definitions. I will take the point with regard to "advance" and "transfer" for instance. The advance of a ship is clearly that amount which she advances in the original direction before turning at right angles. I will read what Captain Colomb says on page 376. "At first I used the co-ordinates for the purpose of analysing the curve, and comparing it with others; and then, for convenience, the co-ordinate was called the 'advance' and the abscissa the 'transfer.'" Those who will turn to Fig. 1 will see the advance goes as far as D, and the transfer is naturally the amount from D to C^2. Those are very important points, because they show the advance which has been made from the place where the helm was first put over, and the transfer from the right or left which the ship has made. Now, I should like to make a few remarks on the nature of the curves. I must fairly say, having had some experience of making these curves, recently especially,—because I was in command of one of the

largest ships in the navy, a double-screw ship—I am afraid, although we conducted the experiments with every care, we did not succeed in making these exact curves as shown in this Paper. I should like to refer again to Fig. 3; in fact, to all the figures. It will be seen that the ship originally makes a curve outwards; that is to say, when the ship is endeavouring to turn to the right it will be seen there is a curve turning slightly to the left. That was the case occasionally, in a very exaggerated degree, with the "Dreadnought." Occasionally, after the first four-point turn, she was absolutely outside the straight line of the ship's course when the helm was put over. That is a very extraordinary thing. I am inclined to give a technical definition of it, but I would rather leave that to the lecturer and to the naval architects here present. It certainly was the case on certain occasions, but on other occasions it was not so. I attributed that to a latent tendency the ship had to turn either one way or the other before the helm was put over. Speaking generally, I may say that our experience bore out that of the lecturer, but I thought it was as well to complete the circles. In certain of the experiments there were very considerable variations, but yet in the end the mean of them was wonderfully similar. The mean of three in one direction, and the mean of three in another, and the mean of the diameters of the circles at various speeds, all came to very much the same thing, curiously enough. Now I know in the "Dreadnought," with a speed of thirteen and a-half knots, the mean diameter of the circle was 416 yards, and I know that the mean diameter of the final circle was 420 yards at eleven knots. The times, on the other hand, varied very considerably, and the greater speed gave us a circle in 4′ 32″, whereas at the lesser speed it was 5′ 17″. Therefore, I think, the general rule holds good, that in putting the helm over at the same helm angle, the ship will make a circle of about equal diameter, whatever the speed, and notwithstanding variations in the speed, but that the time is very materially lessened at the higher speed. That is not entirely in accordance with what was supposed to be the case at first, but I believe that is now generally accepted. With regard to the experiments as carried out in the "Dreadnought," I am happy to say that I carried them out exactly as stated at page 375, and I think it was certainly the easiest way and the most correct way of carrying out those experiments. The buoy was weighted as stated there, and there were observers at each end of the ship, and we found it convenient to dip a flag, which was seen by the observers, fore and aft, and at the moment that flag dipped a bell was rung for the information of the engine room. Of course it will not have escaped the observation of the lecturer, although it may not perhaps be generally known, although it might be assumed, that in turning, the speed of the screws varies considerably. The rapidity is of course reduced with the speed of the ship, but not correspondingly, as stated by the lecturer. The outer screw makes considerably more revolutions than the inner screw. Then there is the question of the area of the rudder, into which I will not now enter. As regards helm angle, with steam steering gear, we found that we could put the helm hard over in something like thirteen seconds, and the helm naturally went over to its number of degrees. I think it was 35° in the "Dreadnought," speaking from memory. One curious fact which I have mentioned in the "advance," is that of the ship going outside the circle on the helm being first put over, and I repeat I should like some account to be given of that by the naval architects. There is some little confusion as to the questions of turning and steering on which I should like to say a few words. There are certain ships which steer very well in one sense and steer

very badly in another. Now, if I might take two ships of the navy, I should take for instance the "Warrior" as a ship that steered very badly, and I should take, I am afraid, one of our recent ironclads, the "Ajax" or the "Agamemnon" as a ship that steers very badly in another way. Those two are often confused together, not by members perhaps of this Institution, but by outsiders. People say, How is this? The causes are evidently entirely different. The cause of the "Warrior" steering badly is that she has a small rudder. The proportion of the area of the rudder to the area of longitudinal midship section of the ship is small, and the ship turns very slowly, but she probably turns steadily. In a ship like the "Agamemnon," in which the area of the rudder is very large in proportion to the immersed section of the middle line of the ship, she does not always answer her helm readily, but when she does begin to turn she turns rapidly, and the only difficulty is to stop her. What I was told as to the "Agamemnon" by the pilot who took her out to Spithead, before certain alterations were made in her, which were made afterwards, was, that when she was on the turn, when he put the helm over the other way, she turned seven points before she could be stopped. I am happy to say that that is very exaggerated, and that certain alterations were made in the trim of that ship and her sister ship, and that fault does not exist now. That is a thing which may occur and does occur from a ship being of a bad form, but certainly not from her having insufficient rudder power. I do not wish to add anything more, except that I feel sure great advantage has been gained by the whole navy, and I feel sure that great advantage will be gained by all nautical men, by the attention paid to this subject of the steering of ships. It is very much better understood now, and the main facts and principles on which ship steering is based are very much better understood now than they were a certain number of years ago. At the same time, I know that we have a good deal to learn yet. Anybody who knows the history of the "Agamemnon" will know that doctors differ. Several people made suggestions as to what should be done with her. Something is being done now to her sister ship, the "Ajax," and I hope it will be a success. I am bound to say I think in that case we must look to the form of the ship, and that the question of altering the rudder does not bear on that particular ship. If I have digressed from the subject at all, I hope to be excused, but there is so much misapprehension upon the question of a ship's steering, that I thought I was justified in alluding to a few of those subjects which were not touched upon by the lecturer.

Admiral Sir JOHN HAY: My Lord and Gentlemen, I wish before Captain Colomb replies, to ask a question of him which, perhaps, being a pre-scientific officer, may not require to be replied to. But I must say I doubt very much the fact, as stated by him, and by my gallant friend just now, of its being possible, as depicted in Fig. 2, that the alteration of the ship's course from the right line on which she is moving, the first time that the helm in this particular case is put hard a-port, is to throw the whole body of the ship in the other direction. It seems to me that an apparent change of that kind is due to the fact that the observers are a long way abaft the movable pivot of the bow, and the action of the helm being to throw the body of the ship and her stern outside the curve, leads them to suppose that the bow itself has gone in the opposite direction. I, myself, do not believe that at the moment of making the turn, supposing it to be in perfectly smooth water, the bow of the ship goes, as in this particular case it is depicted, to the left of the course when the helm is put to port. I am

anxious to hear from those who are better informed than I am, whether there is any absolute proof that that is the case. I think the turning of the ship, which of course is due to the action of the helm on the whole body of the ship, pushing the whole body of the ship to the left of the course indicated, is the cause of the apparent placing the whole body of the ship to the left of the right line, but I do not believe myself it absolutely does take place, as is depicted in this diagram.

Sir Nathaniel Barnaby: My Lord and Gentlemen, I must, with your permission, express my great satisfaction at the interest which naval officers have recently taken in the turning powers of steamships. I am obliged to use the word "recently," although, perhaps, they might take some exception to it, because it will be evident from what Captain Colomb has said that the very start of those investigations to which he has called our attention so ably this morning, was so recent as the year 1877, and that they were conducted by naval architects. I must confess that I have often been, during the course of my official connection with the navy, somewhat surprised at the absence of interest, as it seemed to me, in the question of the turning powers of particular ships by the officers in command of them. I will not say that they did not know the powers of turning of their ships, because probably they did, but they did not give us the information. They knew a great deal which, I dare say, they would have used most ably in command of their ships in action, but for scientific purposes it was not accessible to us, and it was not until 1877 that the more accurate modes of ascertaining what a war ship does in her first and second quadrants came to be discussed at all fully or in an available manner. I should like also to say a word, if I might be permitted, about the "Ajax" and the "Agamemnon," to which Admiral Fremantle referred. I do not think I have ever spoken of the question before this Institution, and I will not take many minutes in saying what I have to say. When these ships were designed they were designed with twin screws placed far apart, and we knew that in giving a ship of that kind twin screws we gave to the officer in command great power of handling his ship, and we treated, therefore, with a little contempt, I am afraid, the question of whether or not the rudder was efficiently placed as to position with regard to the screws. The ships were short, 285 feet long; and heavy, 8,500 tons displacement; and shallow, 24 feet draught of water; and the weights were so distributed in them that we knew that there might be possibly some difficulty in steering by means of the rudder, and on the very first drawings that were made of the ship we showed a three-bladed rudder, because it was our intention to put a three-bladed rudder upon the ships. But the experiments which have been made in England, following those which have been made in France with the Joëssel rudder, which had been fitted in a great many French ships, as we thought successfully, showed us that there was reason to believe that such a three-bladed rudder would offer more disadvantage than advantage, and we were therefore obliged to give up the idea of putting the side blades on. When the proposals were made for modifying the size and form of the rudder, I was opposed to all of them. My own view of the matter was that if a deeper draught of water were allowed the evil might have been cured by putting a keel upon the ship, and it was put to the Lords of the Admiralty: Now, you have a short and heavy ship with a very light draught of water. She has great advantages, but there are disadvantages connected with that: are you prepared to give up the advantage of the shallowness of the draught in order to get greater steadiness in steering? if so, we will put a false keel on, which, we believe, will

result in what you are asking for. Their answer was, No, it may be that the ship would have greater steadiness on her course if she had that keel, but we should have to pay too much for it in the increased draught of water.

Sir JOHN HAY: The Suez Canal.

Sir NATHANIEL BARNABY: The Suez Canal is one of the questions of course in point. When, therefore, a ship can be cured of what is regarded by many naval officers as a very serious defect by such a simple remedy as putting on a false keel, it seems to me that a great deal too much is made of the case. The view of those officers, who said that they would rather have the "Agamemnon" with her twin screws and her unsteadiness on her course than they would have the best steering ship they ever had, with a single screw, will, I believe, be understood, and probably appreciated. I understand that, since I left the Admiralty, a proposal which I made before I left is being carried out in one of the two ships at Chatham. It consists, not in any alteration of the rudder, but in a small prolongation of the stern of the ship above the rudder. It seems to be a curious thing to do, but I and Mr. White have reason to believe it will succeed in giving greater steadiness of course, to the ship. Whether that be so or not, I should like the members of this Institution to understand that in my opinion all the difficulty as to the steering might be avoided if the navy cared to accept the greater draught of water involved in the addition of a false keel.

Mr. ARTHUR J. MAGINNIS: My Lord, having introduced a plan to the Institution last night, which to a certain extent will assist these interesting trials, I should like to mention a few things which struck me on the reading of the Paper. In the first place there has been no mention made of the size of the rudder, and as Mr. White mentioned yesterday, we know that by a very slight change of the propeller of a steamship a wonderful effect can be achieved; that is to say, the speed can be increased, and such like. By the diagram brought before us to-day certain ships are pointed out as answering the helm very badly. I should like to ask if any attempt has been made to alter the rudder, or in some way to try and get to the cause of the bad steering—that is to say, the rudder, because there is no doubt about it that each ship may have a certain rudder which will have better steering power over her than one already on her. All through these experiments, one interesting point has been missed, and that is the strain upon the rudder. I make a special point of this, because I think this has been in all cases omitted, no doubt owing to the fact that no methods have yet been designed for taking the strains at the different angles of the rudder, and I venture to call the attention of the naval officers and the other gentlemen present to the little instrument which I brought forward last night. I do not in any way bring it forward as perfect, having designed it in a very great hurry, and being most anxious to get it to work before this meeting, so that I could obtain some results from it. However, I hope in an Appendix to the Paper, which I trust the Institution will add, to give a full description of it, as I think by a slight modification, that while these trials are going on in the navy this can be applied for the purpose of taking all the strains and registering the effects that are taking place on the rudder itself, and this may in some way help to clear up these matters. Then on the

other point of the much longer time taken to turn the first octant than the others. I think that this little instrument (working with an arrangement of clockwork, instead of the system I adopted) will all the time write down the strains to which the rudder is being subjected, and so we shall have the various strains of the first, second, and the third octant, and so get at a solution of the difference. In my opinion, when ships are going at very high speeds the rudders, when first put over, have strains on them very much greater than when the ship settles down to her turning, as may be noticed by the strain diagrams, which were taken very hurriedly indeed, because in the merchant service we cannot manœuvre the ships as in the navy. Another thing which Captain Colomb brought forward to-day, is the way in which one circle is more quickly effected by turning to the left than by turning to the right. This, I presume, is to be attributed to the effect of a right-handed propeller. I was very much struck with the marked difference in the port and starboard strains as shown by the diagram I had taken, and mentioned in my Paper last night, by putting the helm over a few degrees (I think four), the difference for starboard helm is greater than for port, but after that angle is reached this difference seems to die away, and as soon as the helm came over to the 40 degrees the strains seemed to be exactly the same for the starboard helm as the port helm: whether this would be so or not with a continuous turning of the ship I am not prepared to state, but I think this little instrument would write the matter down, and would tell itself whether such would be the fact or not. Last night I think I misled the meeting by a certain statement which I made, and that was that the strain on the rudder when hard over was $5\frac{1}{2}$ tons; it should have been on the gear, that on the rudder being $16\frac{1}{2}$ tons.

The President: It is quite contrary to our rules, Mr. Maginnis, to discuss a Paper which has been previously discussed; you must not allude to anything that is passed.

Mr. Maginnis: I had pretty well finished, my Lord. In the case of the little instrument referred to, there would of course be some difficulty in applying it in the navy, but I merely mention it because we all know in what a splendid way the navy carries out these experiments, and there is no doubt it can be directly applied, with some arrangement of clockwork such as was brought before us yesterday in the beautiful instrument of Admiral Paris.

Mr. Macfarlane Gray: My Lord, these diagrams are to me very interesting. When I first agitated for steam steering one of my arguments was that, in changing a course to clear an object ahead, the amount of lateral divergence from the original track increases during the critical moments as the square of the time from the instant that the rudder is got over. That statement rested on the assumption that the original track is a tangent to the curve described. Upon this ground I urged the importance of saving time in getting the rudder over, because, although the time saved is at the beginning, the clearance gained measures itself at the end, when the rate of divergence is greatest. The importance of this consideration is beautifully shown on the diagrams now given by Captain Colomb, but the argument I used is there seen to be greatly an underestimate of the magnitude of the advantage of prompt action. The diagrams show that if a vessel is to clear a stationary object ahead of her own

breadth, she must be able to turn herself 45 degrees before she clears it, and the whole amount of lateral divergence of her exposed quarter is accomplished in the last eighth of the time, or, say, eight seconds. Seconds lost at the beginning are marked off in retribution on this part of the curve to a life-or-death issue. It required, say, 60 seconds to clear the object, but only one second less may be catastrophe. These remarks are suggested by this Paper, remembering the recent sudden collision, resulting in the loss of one of our largest mail steamers, and they remind me of a provision I was careful to make in steam steering engines to secure the utmost possible promptitude of action when an obstruction close ahead is suddenly observed; it was that the person whose duty it is to order the helm to be altered had the means at his hand to do it himself, without wasting even the fraction of a second to give an order. As the controlling wheels were arranged according to my plan, there might be one of the wheels near to the officer on the look out, to be used by him in any emergency. This would not interfere with the ordinary steersman; the controlling wheels are made to stand still by themselves, they do not require a man to hold them. The steersman would have to be told that the sea can never move his wheel, and that if the wheel attempts to move he ought to allow it to move, it is wanted to move; the officer on the bridge has taken charge of it for the instant. I remember, when H.M.S. "Northumberland" was first steered by steam, her commander, the late Captain Roderick Dew, on the trial trip, exclaimed exultingly: "I will steer her myself in narrow waters; I do not require even to stand by the wheel; I can walk to the side of the ship and see what is in the way." I have not found this provision has ever been made use of in the way I expected it would, and I make these remarks now, that it may be clearly understood by those in charge of steamers that there is no reason why the officer on watch should not, in emergency, himself move a wheel on the bridge controlling the rudder, if greater promptitude of action would thereby be secured.

Mr. W. Denny: My Lord, may I ask Captain Colomb if he has made any experiments with what we call, in the merchant service, supplemental rudders? My firm has built a large number of steamers running between this country and the East, which have to pass through the Suez Canal, and it has been found in these steamers that the best way to get over the difficulty of steering in the shallow water, and at the slow speed required in the Canal, is, not to increase permanently the size of the rudder, but to add a supplemental rudder for use in the Canal alone. Are there not other conditions where it would be advisable to have such a supplemental rudder, instead of making changes which, while very good for special circumstances, are not required in general work? It is conceivable that in some war steamers a certain kind of rudder might be wanted for ordinary slow speeds in cruising, and something different for high speeds and for going into action. I think the expedient of the supplemental rudder which has been found so successful in merchant steamers might be found worthy of consideration in the Royal Navy. It is possible that the matter has been considered at the Admiralty and investigated. In any case would it not be advisable to try some of them on ships of the Royal Navy? In these ships there are many opportunities of making very exhaustive and very thorough trials, not only with their ordinary rudders, but with various forms of supplemental rudders. These supplemental rudders are very cheap things to make, and the permanent fittings required for them or the main rudder are in no way detrimental to ordinary work.

Mr. White: Would your Lordship allow me to make one statement? It is of an interesting nature, and is simply a statement of fact. Captain Colomb has given us a practical method—a seaman's method—of noting the turning. I know, as a matter of fact, and I daresay Captain Colomb knows, that in both the French and English navies at the present time officers of considerable scientific attainments are working out a gyroscopic method by which a ship will be made to plot her own path in turning without anybody intervening at all.

Captain P. H. Colomb: My Lord, I think we must all agree that the discussion has been exceedingly interesting and to the point, if I might be allowed to say so. The first remark I have to answer is this: Mr. White spoke of this being a sailor's matter, and I had noted the expression that he used with a view rather of contravening it, until I heard what he said at the last. His view is, and I quite agree with him, that it is in the first instance a sailor's matter. It is the sailor's business to collect the data and to furnish the naval architect with it, in order that the naval architect may go on continually improving the manœuvring powers of ships. It is, therefore, a sailor's question in the first instance, but I may be allowed to believe it is a more important question for the naval architects afterwards. Admiral Fremantle thinks my former method of employing the co-ordinates for the analyses of the curves is better than my present system of chords and angles. It may be so, of course, but the chord and the angle seem to me to be a very easy and simple way of carrying in your mind the sort of curve the ship makes. You notice in Fig. 2 the 12-point turn, that is, the curve from C to C^3 is the longest—or nearly the longest, of the whole—very commonly it is the longest. There is an advantage, then, in saying that the chord of 12 points is the actual measure of the space that the ship requires to turn in, that this ship is a 6·26 chord ship and that her angle is 52 degrees. You thus get in your mind at once a better analysis of the actual powers of the turn than if you took the ordinate and the abscissa at the point C^3. Perhaps it is not so scientific as the use of the co-ordinates, but, on the whole, I find it better for practical comparisons. Admiral Fremantle also drew attention to the difficulties of observation: there is no question but that those difficulties do exist; it requires a little training. If you take a fresh set of officers and put them in the different positions for observing and noting, your first turn is almost certain to be a failure, but after one or two turns, when they get in the way of it, the observations become exceedingly accurate, as is shown in Fig. 2, and Fig. 2, I should point out, is not an exaggerated case at all, it is quite common to get three or four runs where the difference in the observations are exceedingly small.

Admiral Fremantle: I did not object that there was any great difficulty in taking the observations, but that the observations, after they were very carefully taken when they were plotted out, did not give in all cases similar results, and that there was occasional eccentricity in the movement of the ship. That is what I wish to say.

Captain Colomb: All I know of it is against that. All I know about it goes to point out that the movement of a ship under similar circumstances in the water is almost as if a groove had been cut for her to turn in. Where there are eccentricities, they generally seem to me to result from defective

observation, and not from any variations in the movements of the ships themselves. I have many examples closely resembling Fig. 2, and I have no reason to doubt the very accurate movement of the ships in the water. Admiral Fremantle drew attention to a very important point indeed, which, I think, is apparent, if my Paper is closely looked into, that is to say, that we are a little confused as to what is good and bad steering. A ship in some cases will steer, as you think, very well, and in other cases apparently very badly; that is to say, what a sailor will often call a handy ship when he takes her in and out of harbour and manœuvres her in close waters may not really be a handy ship. She may answer her helm quickly in turning through small angles, but very slowly when she comes to turning through large angles. I quite agree with what has been said, that we have a great deal to learn. In fact, we are only just on the fringe of the question, and are just beginning to understand a little about it, and by-and-bye we shall understand a great deal more. Sir John Hay criticized the drawing of these curves as showing a passage of the ship to the left of the original course at the first part of the turn. I should say that direct observations have shown that this is correct. It has been confirmed by a great many indirect observations that portions of the ship—it does not follow that the whole ship—passes to the left of the original course. If I am not wrong in my recollection, Mr. White, in a Paper he read before the United Service Institution, pointed out that it would be quite possible that the whole ship would pass to the left of the course in the first instance. That is, that what we call the "pivoting point"—the point about which the forces are acting—may be ahead of the ship altogether at the beginning. There is no doubt that the pivoting point travels; it may travel forward at first and backward further on, until it settles down into its fixed position, when the curve which the ship is describing is circular, but I think the cases would be rare when the whole ship would pass away to the left.

Sir JOHN HAY: Is that due to the following current?

Captain COLOMB: No; it is due chiefly to the moments of inertia, to the position of the centre of gravity, and the way the moments of inertia affect the rotatory movement at first.

Mr. WHITE: It is really a question, putting it in mathematical language, of the rate of acquisition of angular velocity.

Captain COLOMB: I am glad to have Mr. White to support me: it is a question rather beyond me. Sir Nathaniel Barnaby spoke of the little that naval officers have done until late years, and expressed his surprise at it. Well, the fact is, I think, that naval officers have been working at it off and on for a very long time, but of course it does take a long time to develope accuracy. I must say here that I think upon all these questions we are greatly indebted to the French naval architects and naval officers. They are nearly always ahead of us in the scientific part, although I believe myself that when we have hold of the scientific part, we beat them ultimately in the practice. I agree very much with what Sir Nathaniel Barnaby said about the "Ajax" and the "Agamemnon." I felt all along, with regard to those two ships, that the false keel he spoke of would most probably remedy the defect, and that there never would have been that defect if there had been a false keel. As to what is going to be done with them now for

the purpose of remedying the defect, it is a curious and interesting question. Mr. Maginnis said that no mention was made in my Paper of the size of the rudder, whereas the whole Paper goes to show that the relative area of the rudder, as compared with the area of the immersed section, is one of the elements taken into consideration, and given in the Table at the end. Then this curious point comes out—rather, I think, in opposition to what he has said. If you look at Fig. 3, No. 3 Ship N, in the third line of the Table, you will find that her co-efficient of rudder power is ·0189, that is to say, it is very small compared to the others in the same column, but if you turn from that to the Fig. 3 you will see that the ship which appears to have the smallest proportion of rudder area is the best turning ship that we have amongst our diagrams. That is to say, the rudder area, although an element which you must take into account, is only an element. I believe myself that form of rudder, especially form as to relative depth and width, is even more important than area, but I am certainly satisfied of this, that ultimate helm angle is much more important than rudder area itself in most ships. I think the instrument he speaks of as automatically recording the rudder strains would be a most interesting and most useful one to apply in all these trials of ships. I quite go with him there. I think it is an excellent idea, and a very valuable one indeed. Agreeing so very strongly as I do with so much that Mr. Macfarlane Gray has said, I regret very much that I disagree with him (and I feel myself very small in disagreeing with a man of his great genius) on one particular point. If I understand him aright, he supposes that the speed of the helm movement is a very important element in the turning power of ships. As to the value of the steam steering gear as adverted to by Mr. Macfarlane Gray, there is not a naval officer anywhere in the world who will not go thoroughly with him. Mr. Denny asked whether I have made any experiments with the supplemental rudder. I have no knowledge of the subject at all. Mr. W. H. White drew attention to the automatic gyroscopic arrangements which are being prepared in our own and the French navies. There is no doubt whatever that that is a line of invention which, if followed out, would do as much as anything possibly could do towards enabling us to gain that complete knowledge we want to obtain of the turning powers of ships.

The President: I am sure, Gentlemen, you wish me to return your thanks to Captain Colomb for his valuable Paper, and I am quite sure Captain Colomb must feel gratified with the discussion which has followed upon it. We are all grateful to those very eminent naval officers who have contributed their valuable aid to this discussion. I now return, on your behalf, thanks to Captain Colomb.

ON THE STRENGTH OF BULKHEADS.

By T. C. Read, Esq., Member.

[Read at the Twenty-seventh Session of the Institution of Naval Architects, April 17th, 1886; the Right Hon. the Earl of Ravensworth, President, in the Chair.]

The great additional safety derived from dividing steam vessels into numerous water-tight compartments is now appreciated by shipowners, and sub-division by cellular double bottoms and by bulkheads is every day becoming more general, and were it not for the disadvantage of broken stowage, which necessarily accompanies the fitting of bulkheads, would no doubt be even more extensively adopted than at present. It is evident, however, that if inner bottom plating and bulkheads are not sufficiently strong to resist the stresses thrown upon them when the outer skin is broken, their existence would more probably be a source of disadvantage than of advantage to a vessel. In the present Paper a method is proposed for finding approximately the stress on these parts of the structure when under water pressure, and the somewhat laborious nature of the calculations will, it is thought, be justified, if the efficiency of the bulkheads, &c., is proved, or the parts under excessive stress be found, in order that steps may be taken to render them sufficiently strong to successfully withstand the stresses thrown upon them.

When the outer skin at the bottom of a vessel fitted with water-ballast tanks is broken through, and the vessel is consequently floating on her inner bottom, the stress on the inner bottom plating is largely dependent upon the weight of cargo on the inner bottom, and the resultant force may either be upwards or downwards, according as the weight of the cargo is less or greater than the upward pressure of the water on any given area. The most critical case probably arises when there is no cargo in the hold in way of the damaged bottom, and where consequently, the whole pressure of the water has to be resisted by the stresses on the inner bottom plating.

The case of a vessel fitted with water-ballast tanks, formed by deep longitudinal girders on top of the ordinary floors, is the most simple, and will be dealt with first. The plating, which is generally $\frac{6}{16}$ in. thick, is fitted on the top of the girders, which are spaced about three feet apart, and the girders are connected to the reverse

frames, and to a short lug fitted on the top of the floor plate on the opposite side to the reverse frame, by two rivets at each floor plate.

In some of the earlier vessels of this type there was only one rivet at each floor plate, and the tension on this rivet would be equal to the water pressure on six square feet of the inner bottom plating. With the inner bottom 15 feet below the water surface, this would amount to nearly $2\frac{1}{2}$ tons, and would, in the case of a damaged outer bottom, be considerably increased by the up and down motion of the vessel.

As these rivets were often found to be loose or broken, lug pieces are now always fitted, and double the amount of rivet connection obtained.

The inner bottom plating being unsupported between the side girders would bend upwards under water pressure, if free to move at the ends, and for the purposes of this calculation its resistance to bending has been neglected, and the whole pressure assumed to be resisted by the tension in the plating.

It is difficult without experimental data to say what this resistance to bending would amount to, but as the value of the greatest possible stress on the weakest part is sought for, an extreme limit is obtained by supposing the thin plating to be flexible.

If h be the distance below the water surface, the pressure on a foot length of a transverse strip of the inner bottom plating one foot wide would be $\frac{h}{35}$ tons. Also if T be the tension on such a strip, and θ its inclination at one of the side girders, then—

$$2\,T\,\mathrm{Sin}\,\theta \text{ or } 2\,T\,\theta \text{ since } \theta \text{ is very small} = \frac{3h}{35} \qquad \text{(I)}$$

the distance between the side girders being taken as three feet.

Further, if R be the radius of curvature of the plating—

$$R\,\theta - R\,\mathrm{Sin}\,\theta = R\left\{\theta - \left(\theta - \frac{\theta^3}{\underline{3}} + \text{ \&c.}\right)\right\} = R\frac{\theta^3}{6}$$

is one-half the extension of the plating between the side girders. Hence, if A be the sectional area of the strip considered, and E the modulus of elasticity—

$$T = EA \cdot \frac{R\,\frac{\theta^3}{6}}{R\left(\theta - \frac{\theta^3}{6}\right)} = EA\,\frac{\theta^2}{6} \quad . . \qquad \text{(II.)}$$

From this equation, and equation I, it follows that—

$$T^3 = \frac{E \cdot A \cdot h^2}{9800}$$

and if E be taken as 10,000 tons, $h = 15$ ft., and the plating $\frac{6}{16}$ in. thick, T = 14·6 tons, or

about $3\frac{1}{4}$ tons per sq. in. The shearing stress on the $\frac{5}{8}$ in. rivets, spaced $2\frac{1}{2}$ in. apart in the longitudinal seams, is slightly more than three tons per rivet, and thus the stresses in this case are well within the power of the material to withstand.

Turning to the case of a vessel with a bottom framed on the cellular system, the inner bottom is supported longitudinally by the side girders, spaced from 42 in. to 54 in. apart, and transversely by the deep floor plates, spaced $3\frac{1}{2}$ to 4 ft. apart. The water pressure is here resisted by longitudinal as well as transverse tension on the inner bottom plating, and to find the actual amount of these tensions it is necessary first to find the amount of pressure borne by each at every point. To do this the following course has been pursued :—As before, if p be the pressure on a foot length of a strip of the plating one foot wide, T the tension on the strip, and θ its inclination to the horizontal, then, at any point, by resolving vertically—

$$\text{T Sin } (\theta + d\theta) - \text{T Sin } \theta = p.\ dx.$$

or, since θ is very small, $\text{T}d\theta = p.dx.$, and if $d\theta$ be the increase in θ per unit length, then since T is constant, p varies directly as $d\theta$.

Now consider the case of a transverse strip of plating midway between the frames. The pressure close to the side girders borne by this strip must be the whole water pressure, since there is no deflection and consequently no longitudinal tension on the plating at that place. Midway between the side girders, which are assumed to be 4 ft. apart, the pressure is one-half the total water pressure, since the condition of this strip is identical with that of the longitudinal strip, midway between the side girders (that is A B and C D, Fig. 1, Plate XLVI).

Again, if the condition of the strips G H and E F (Fig. 1) be considered, it follows from symmetry that at O the pressure borne by each is equal to one-half the total pressure and at the points E, G, F and H the whole pressure is borne.

Now take L M (Fig. 2) = G N (Fig. 1) = 2 ft. and set off L R a convenient distance to represent $d\theta$ for the strip G H, at G, and also P Q to represent its value at O, therefore L R = 2 P Q.

Also set up T U (Fig. 3) to represent $d\theta$ for the strip AB at B, and SW to represent its value at K, then T U = 2 S W.

If, now, any value be assumed for the pressure borne by the strip GH at N, say $\frac{1}{4}$th total pressure, and M I be made = $\frac{1}{4}$ R L, a curved line R Q I (Fig. 2) can be drawn giving

the value of $d\theta$ for each point in the length of this line. And since $\frac{1}{4}$th the total pressure is assumed to be borne by GH at N, $\frac{3}{4}$ths must be borne by the strip at AB, hence make $A_1B_1 = \frac{3}{4}$ UT and drawn the line UBW which will give $d\theta$ for each point in KB. Now, by the ordinary graphic process integrate each of these lines, starting from M in Fig. 2, since at the point N the value of θ, for the line GH is = O, and from S in Fig. 3, since the tangent is horizontal at K. Lines MCD, SEF, are thus obtained, giving the value of θ in each case, and since dh, the increment of the linear deflection, $= dx$ Tan $\theta = dx.\ \theta$, θ being very small, second integrations will give lines representing the linear deflection at each point.

Again, since ds the increment of extension of plating at any point $= \frac{dx}{\cos.\ \theta} - dx = dx.$ $\left(1 + \frac{\theta^2}{2} - \frac{\theta^4}{\underline{|4}} + \text{\&c.} - 1\right) = dx\ \frac{\theta^2}{2}$, if a line representing $\frac{\theta^2}{2}$ be plotted and integrated, the result ($= s$ say) is the total extension on a 2 ft. length of plating and $T = E\ A\ \frac{s}{2}$, A as before being the sectional area and E the modulus of elasticity.

Now suppose TU $= a$ the assumed value of $d\theta$ at B, then if x be its actual value, the correct value for the extension is

$$\frac{x^2}{a^2} . s \text{ and } \therefore T = E\ A. \frac{x^2}{a^2}\ \frac{s}{2}$$

and at the point B since T $d\theta$ = the whole pressure :

$$E A \frac{x^2}{a^2} . \frac{s}{2} \times x = p.$$

From these equations T and x and the correct scale for each of the curves in Figs. 2 and 3 can be found, and the values of h at the point N can be read off from each diagram. If these values are unequal, different values must be assumed for $d\theta$ for each of the strips at N; a second or third trial will give the correct values with accuracy.

The values for $d\theta$ for each of the lines AB, CD, EF, GH, LM, PQ (Fig. 1), can now be plotted, and the tensions along each of these lines being known, curves may be drawn showing the variation of the stress from girder to girder and from floor to floor, over the whole surface. In the case calculated, the floors and girders being each 4 ft. apart and the inner bottom 15 ft. below the water surface, the maximum tension per sq. in. on the inner bottom plating is 3 tons, and the maximum shearing force on a $\frac{5}{8}$ in. rivet in the seams $2\frac{3}{4}$ tons. Diagrams of stress on the plating over the bay are shown by lines on Fig. 1.

The case of a bulkhead under water pressure presents some points of difficulty that do not arise in estimating the stresses on inner bottom plating. In the first place the water pressure is not uniform over the whole surface, but increases with the depth below the water line, and further, the depth and breadth of the bulkhead not being equal and the addition of stiffening bars increase the labour of estimating the stress in this case.

The same general method has, however, been adopted and varied to meet the conditions of the problem. As by Lloyd's Rules a vessel above 40ft. broad requires a vertical web plate to be fitted amidships to stiffen the engine-room bulkheads, the bulkhead most severely strained when under water pressure will be one falling just short of this breadth; for convenience, I have taken the breadth as exactly 40ft. and the depth from middle deck beams to top of floors 20ft. The water has been assumed as rising to the height of the middle deck beams. This is perhaps rather a less height than would occur in a fully laden, full scantlinged vessel, but is about correct for a spar-deck vessel. Of course an increase in height of water line would cause an increase of strain in the bulkhead, but would not affect the method of calculation.

A vertical strip of the bulkhead plating at the middle line is first considered, and its deflection and tension found on the supposition that it bears the whole water pressure. Proceeding as in the previous case the line representing the pressure, in this case a straight line (A C) Fig. 4 is drawn. As before, this line represents ($d\theta$) on a scale that can be found when the tension is known. Integrating this line from the top, a line (A D) representing θ is found, and a second integration gives a line (A E) for h. It is now necessary to introduce the "constant" required by the integration. This is easily found thus:—, Since at the bottom of the bulkhead the linear deflection is nil, if the points A E be joined by a straight line, the ordinate of a straight line A E will give the amount to be deducted from the reading of the curved line A E, at any point. That this is true can be seen thus: a "constant" should be added after the first integration (C say); after the second integration this term will appear as Cx, and thus would vary directly with the depth, and its value at the bottom would be such as to make the value of $h = o$; if therefore the horizontal distances between the straight and curved lines A E be set out horizontally from A B, a curved line A F B is found which will be the form of the vertical strip under the whole pressure of the water.

The angular deflection is then found by drawing a horizontal line through the point of A F B where the curve is parallel to A B, to meet A D in G. At this height the value of θ is O, and hence if a vertical line K L is drawn through G, the ordinates of the line

A D, measured horizontally from K L, will give the angular deflection at any point. The point G may also be found thus :—Since K A is the value of θ when $x = o$, K A = C as above, and also C $\times$ 20 = B E as above explained, and hence K A is known in relation to B E the scale of the curve being of course corrected for.

Having found the value of θ at any point, a curve representing $\frac{\theta^2}{2}$ is plotted and integrated, and thus the extension, and consequently the tension of the strip with the assumed deflection found.

If now the correct value is given to $d\theta$, which is done by making $T d\theta$ = pressure, the scale of the curves is found and the actual values of the tension and deflection at every point.

At the bottom of the bulkhead, since there is no horizontal deflection, the whole pressure is borne by the vertical strips from side to side of the vessel. Hence in the example taken, $T d\theta = \frac{20}{35} = \cdot 572$ tons, T between the tension on a strip 1 ft. wide; also at the top of the bulkhead $T d\theta$ is 0, from side to side of the vessel. Now assume different values for that part of the pressure taken by the tension on the vertical strips at each point of the height of the bulkhead; or in other words, draw curved lines A N C, A P C to represent the values of $d\theta$. As the pressure on the horizontal strips varies roughly as the cube of the deflection, these curved lines can be made to closely approximate to the actual form of the line for $d\theta$, at some point of breadth of the vessel. Thus at the top of the vessel it will conform closely to A C, be nearly parallel to A C at the mid-height, and be curved outwards somewhat sharply at the bottom.

For each of these lines a value of T and the deflection at any point can be found, and with the three values obtained, a curve can be drawn giving the relation between the deflection at mid-height, and the pressure borne at that point. (E F G Fig. 6).

Next a horizontal strip at mid-height is considered. At the sides of the vessel, since there is no vertical deflection, the whole pressure is borne by the horizontal strips, and hence $T d\theta = \frac{10}{35} = \cdot 286$.

It is next assumed that at a point say 5ft. from the side of the vessel that three-fifths of the pressure is borne by the horizontal strips; a line for $d\theta$ is then plotted and integrated. (*See* Fig. 5.) This line is varied until $T d\theta = \cdot 286$ at the ends and the deflection at the middle line is

·336 the value obtained from the vertical strip bearing the whole of the pressure, it having been found that at the middle line the value of $d\theta$ for the horizontal strips is $=0$. If three such lines be found corresponding to different values for the pressure borne at the point 5 feet from sides, a line A B C can be plotted on Fig. 6 giving the relation between the deflection and the pressure at that point considered. Where the two lines A B C & E F G intersect, the value of h and pressure can be read off and the correct lines for $d\theta$ and h plotted for both horizontal and vertical strips.

Horizontal strips at one-fourth and three-fourths depth are then similarly treated and the vertical line varied if necessary until agreement is obtained, at each point calculated.

Fig. 7 shows the result of this calculation, the deflections shown by the full lines, being magnified ten times, and the tensions shown by full lines, at each point both vertically and horizontally.

The above, however, does not include the effect of the stiffening bars. By Lloyd's Rules such a bulkhead would require to be stiffened by vertical angle irons placed 30 ins. apart on one side, and by horizontal angle irons on the other side spaced not more than 4 feet apart. The effect of the stiffeners is two-fold, first by increasing the area under tension, and second by their resistance to bending. The first correction necessary is easily made, amounting only to a change of scale in the previous curves, and if the stresses on the unstiffened bulkhead are not required, the increased area can be used in the first calculation. The correction due to the resistance to bending for the vertical bars is also readily made, since the curved form of the bars bending under a pressure increasing with the depth closely agrees with the form of the bulkhead unstiffened under pressure, and further its effect is small, owing to the small moment of inertia of the section of the bars and plating which may be supposed to act with them. The horizontal bars present a case of greater difficulty, as in this case the bars are bent near the ends round a comparatively sharp curve and are practically straight near the middle line. Details of the work in this case are shown in the Appendix. For each of these bars the pressure causing deflection will vary as the deflection; for the bulkhead plating the pressure varies practically as (deflection)^3 and the corrections for the bars can readily be made by using this relation.

The corrections being all made the diagram of deflection and stress is shown by the ticked lines on Fig 7. It will be seen that the vertical stress amounts to as much as 6·1 tons to the inch at the middle line. It must, however, be borne in mind that these calculations are made on the assumption that no deflection whatever occurs in the beam at

the middle deck on top of the bulkhead. There can be little doubt that even in the cases where the bulkhead extends to the upper deck, and thus with the middle and upper deck plating forms a girder above the middle deck across the vessel, some amount of deflection in the beam does take place. If however the beam has any considerable amount of round up, a downward deflection at the middle would cause a thrust outwards along the beam, this thrust would again be taken up by tension in the bulkhead plating, and thus the deflection of the beam resisted. The same cause would operate to prevent any upward deflection of the floors, and generally any vertical stress taken off, would be counterbalanced by additional horizontal stress.

Another cause for diminution in the stress to be borne by the plating is the initial condition of the bulkhead. It is certain that in all bulkheads some amount of deflection could take place before any tension is thrown on the plating, and this would have the effect of increasing the deflection and thus reducing the tensions.

There is further one other point that should be considered, namely, the deflection of the plating between the stiffeners ; the effect of this is, however, very small, seeing that the stiffeners are only 30 ins. apart horizontally and 4 ft. apart vertically, and that the actual effective pressure producing the deflection does not exceed one quarter of the total pressure of the water ; the increase in the vertical stress is rather less than a half a ton per square inch on this account at the bottom of the bulkhead, and decreases gradually to nothing at the top.

If the upper edge of the bulkhead were released the deflection would of course be greatly increased, and the vertical tension at the middle line would gradually decrease from its value at the top of the floor plates to nothing at its upper edge. This tension would in fact be spent in producing vertical displacement of the bulkhead plating, but it is difficult to estimate the amount of the vertical displacement and consequent vertical tension at any point. From some approximations I have made, it appears that from a point a little below the mid-height upwards the whole pressure may be assumed to be borne by horizontal tension in the plating, and below this point the pressure is, as in the previous case, divided between the horizontal and vertical tension. On this assumption, which I believe is very close to the truth, the maximum horizontal tension is about seven tons, instead of three tons when the bulkhead is rigidly held at the top.

From these results, allowing for the deflection of the beam at the top of the bulkhead, I have estimated that the maximum tension under the condition of practice does not (even in the

case where there is considerable rise of floor and round of beam) reach four tons per square inch on the plating, nor six and a half tons shearing stress on the three-quarter inch rivets at the edges of the bulkhead. As these rivets are subject to double shearing force, with an ultimate strength of eighteen tons, the bulkhead should, if constructed and stiffened as now required by Lloyd's Rules, be well able to stand the stresses under the condition assumed, namely, that of a compartment being filled with water to the main deck.

The bulkhead for which these calculations have been made is, as before stated, that on which the stresses would reach their maximum value, and in cases of bulkheads of less breadth and depth the stresses would be less, as the area under pressure would be smaller and the water pressure less. For all cases where the breadth approaches twice the depth, a sufficiently near approximation to the maximum stress can be readily found by taking the whole pressure amidships as being borne by the vertical tension, and the resistance to bending of the vertical stiffeners, making allowance as before for the deflection of the beam at the top of the bulkhead. It should be noted, however, that in one-decked vessels, where the resistance to deflection of the beam on the top of the bulkhead is small, the vertical stress would be considerably decreased, and may in some cases be less than the horizontal stress at the lower part of the bulkhead.

In conclusion, I desire to point out that I do not claim absolute accuracy for the values of the stresses here obtained, but that the results are fairly approximate, and I trust they may be of some interest to the members of this Institution.

APPENDIX.

Resistance to Bending of Horizontal Stiffeners.

The form of the bulkhead in the line of these stiffeners being flat near the middle line and curved at the sides, it is required to find the pressure necessary to bend the stiffeners round the curved ends, while they are held to the bulkhead at the middle line.

Since there is little or no curvature near the middle line the moment, over the flat part, of the forces acting on the stiffener must be $= o$, and there can be no shearing force there.

Let P be the force on the end of the stiffener; the moment of P is shown graphically by the straight line BC. See Fig. 9, Plate XLVI.

Next assume EFG a curve of pressures on the stiffener, the ordinate at any point being equal to the pressure borne by the stiffener at that point; then, since the shearing force at and near the middle line is = *o*, the area of the line EFG above AB must be equal to the area below AB + the value of P, hence, if this line be integrated (*see* BHI), AI = P.

If now BHI be integrated, a line BKL is obtained, giving the value of the moment of the forces acting on the bar; this line will coincide with the straight line BC, near the middle line, if the scales be made the same, as shown by MNB. The area between BC and BMN is next integrated, this being the bending moment actually producing deflection on the bar, and a line, BOT, for angular deflection found; a second integration starting from the middle line, where the angular deflection=*o* gives the actual form of the curved bar (PQR). If this curve agrees with that of the bulkhead when under a pressure such that the total pressure on bulkhead and stiffeners is equal to that of the water, the necessary correction is made. If these lines do not agree the pressure line EFG must be varied until a practical agreement is obtained.

DISCUSSION.

Sir Nathaniel Barnaby: My Lord and Gentlemen, there can be no doubt, I think, as to the interest which a Paper of this kind has for the members of this Institution. I do not know whether those who have heard it read to-day have been able to follow the mathematical processes; I have not myself, but the general conclusion to which one would come from the reading of the Paper is that the arrangements that are laid down by Lloyd's Committee are such as to make bulkheads in merchant ships trustworthy. I am afraid that a very general impression prevails that very early after an accident has happened to a ship at sea, which floods a large compartment, the giving way of the bulkheads is to be feared. I know that amongst marine engineers that fear has been very largely entertained, and it may perhaps account for the looseness with which bulkheads themselves are placed in the ships and cared for. If Lloyd's surveyors are giving, as one would gather they are from the reading of this Paper, really satisfactory bulkheads, so that we may say to the captain and to his officers, you may trust those bulkheads as you trust the outer bottom of the ship, a great point would be gained. We know exactly what is going to happen in that case. If you have a hole in your ship and the compartments are filled with water, do not be afraid of your bulkheads. It is quite certain that ships ought so to be built that this can be said to the officers who command them at sea; and it would be always a great source of gratification to them if they could so place confidence in their bulkheads. The investigation which we have had before us this morning is a new one. It was undertaken probably for the first time to ascertain what happens in the bending of the bulkheads, and I shall read it myself with a great deal of interest. I will only ask this question: that the reader of the Paper or Mr. Martell would be so good as to tell us whether in their experience it does happen

that the bulkheads give way when water enters the compartments of a ship. I know how difficult it will be for them to answer, because I know what generally happens is that when water does get through the outer bottom of the ship, and is not under control by the pumps, then, whether the bulkheads are strong enough or not does not a bit matter, because there are not enough of them, or they do not go high enough, and the question whether they are strong enough is practically not often raised, but it must have been raised in some cases which would come within the knowledge of the reader of the Paper or Mr. Martell, and if they can favour us with anything which would go further in the direction of giving confidence to all of us, that the bulkheads which are put in are strong and will not burst, it will give us great satisfaction.

Mr. B. Martell: My Lord, I am sure that the members of this Institution are greatly indebted to my colleague, Mr. Read, for producing this Paper. As Sir Nathaniel Barnaby has said, it is the first time that this subject of the strength of bulkheads has really been treated in the scientific manner in which Mr. Read has treated it. We have heard frequently, too often in fact, the assertion that bulkheads give way under water pressure and are useless. Now, the result of this investigation is to show, that from the manner in which the bulkheads are now constructed, and required by Lloyd's Register, they are sufficient in strength to withstand the strain that is brought upon them when a compartment is filled with water, and we have had practical illustration of that in the case of a large ship which had the bottom forced in and the water filling the large compartment of the forehold, and the ship has arrived in port in perfect safety without any damage to the bulkhead whatever. I may say that in consequence of losses occurring, owing to the want of a sufficient number of bulkheads, and their being extended sufficiently high, we have recently extended the requirements in that direction in ships classed in the Register of Lloyd's, and we have to express our very great thanks to the Admiralty for the assistance they have given to the Mercantile Marine in enabling us to accomplish this. The owners of merchant ships had very great objection to the division of their ships, fearing that it would interfere with the loading of them, and we had much difficulty to contend with in divergence of opinions, in getting such a number of bulkheads as appeared to be desirable. But when the Admiralty in their, as I consider if I may say so, very wise action, placed merchant ships on their list to qualify them to be taken up by them in cases of emergency in war, the result of that in requiring a sufficient number of bulkheads, has given us very great support in extending our requirements, and making merchant ships capable of floating in the event of any one compartment being filled with water, which I consider a very necessary requirement. It is a great source of satisfaction to me to find one of my colleagues capable of investigating the subject in the scientific manner he has done, showing us, irrespective of the vague remarks that are sometimes made, that bulkheads are useful for the purpose for which they are intended. If bulkheads be built in accordance with the requirements of Lloyd's Register, they are sufficiently strong, as shown by Mr. Read's investigation, to withstand any pressure that may be brought upon them by the filling of the whole of one compartment with water, which doubtless they should be capable of doing.

Mr. W. Denny: Might I ask Mr. Martell if in the case of the vessel to which he referred there was cargo on both sides of the bulkhead?

Mr. MARTELL: There was not.

Mr. DENNY: The holds were empty?

Mr. MARTELL: Practically empty.

Mr. W. DENNY: I think, my Lord, such a Paper as we have had from Mr. Read would require not only to be heard, but to be studied for one or two evenings before being discussed. I believe the Institution will excuse any one rising to speak upon it, for not being perfectly able to discuss all the points in it, as fully as they deserve. I join with Sir Nathaniel Barnaby and Mr. Martell in expressing my gratification with this Paper, and that this investigation as to bulkheads has been undertaken. It is like some of our investigations—a piece of work which must be developed step by step, and as to which we cannot hope all at once to arrive at a conclusion. I remember Mr. Read bringing before us on a previous occasion, along with one of his colleagues, a Paper on the transverse strength of steamers, in which there was the same feature of beginning an investigation, and I would call the attention of the Institution to the hopefulness of such investigations. Although we cannot all at once get from them definite solutions, we get beginnings, and it is very important such should be made and with every encouragement. In the earlier periods of the Institution we were rather anxious to get our successes at a leap, and as a consequence we neglected many lines of investigation in which immediate success was impossible, but which were very fruitful in promise. I was much pleased to hear Mr. Martell recognize as he did the debt we of the merchant service owe the Admiralty, and especially to Sir Nathaniel Barnaby, for the better arrangement of watertight bulkheads. I would recognize further the indebtedness of my own firm to the Admiralty for an improvement which they were the first to introduce in the construction of bulkheads, and by which I think you will admit some advantage has been obtained. The improvement I refer to is in framing water-tight bulkheads with horizontal plating and vertical stiffening bars. There can be little doubt that when a strain is brought upon a bulkhead the landings must be much better supported by the crossing of the angle bars than they are where the angle stiffeners run in the same direction as the plates. I understand that Mr. Read in the investigation which he has conducted has confined himself to the condition in which the stiffening angle bars run vertically with the vertical plates. This he has very properly done, as it is the ordinary and worst condition. As, however, Mr. Read has considered the parallel case of the inner bottom, with stiffeners running in the same direction as the plates, and also the case of the cellular inner bottom, in which there are cross stiffeners, it would, I believe, be a great advantage if he would consider this case, which is parallel to that of the cellular double bottom, and in which the bulkhead stiffening bars run across the edges of the landings. I believe we might find out that instead of requiring, as we sometimes do, to increase the scantling of bulkheads, we would obtain sufficient strength by a little modification in arrangement. I would further suggest to my good friend Mr. Martell, whom I have often criticized in the past and whom I dare say I shall often criticize in the future, what a debt of gratitude constructors would owe him, and what a feeling of confidence they would have in Lloyd's, if every now and again some investigations were brought out by means of which the structures of steamers could be lightened. It is a mistake to suppose that our one possible line of advance must involve increase of

structural weight. There is as great a line of progress in the direction of lightening the structure, which I think Lloyd's are a little apt to neglect. I believe with the able staff my friend has at his command, it would be as easy for him to adopt this line of investigation as the other. I must again thank Mr. Read for the work he has done on this occasion, work which is so hopeful and promising that we should all welcome it very heartily.

Mr. Arthur J. Maginnis: My Lord, I should like to refer to the matter of the bulkheads as, within the last six months, I have had practical experience of the utility of them. I may mention that the ship is built to Lloyd's Register Rules. It was a question of where to replace the propeller shaft of a large steamer which was afloat. The ship being laid up and docks not being available at once, it was suggested to tip the ship, and after some consideration we were allowed to proceed with the work. The first thing we did was to fill up the fore peak of the steamer. We filled this peak up to a height of 27 feet, and allowed No. 1 hold to stand for about twelve hours to see how the bulkhead would hold the water. The next morning, on examination, we found the water had not risen one inch in the next compartment. We then proceeded to fill up No. 1 compartment—the first compartment was the fore peak. We then filled up that compartment to a height of about 18 feet, and allowed that to stand for another night, and on examination the next morning we found that there was not a quarter of an inch of rise of water in the compartment. On the next day we filled up No. 2 compartment to a height of 20 feet. I may mention that the full width of the bulkhead was 39 feet. By the time we had done this we found that we had tipped the ship sufficiently to get the stern shaft out, and in order to test the thing thoroughly, so as to have perfect confidence in it, we allowed the ship to stand in that way for six days, and at the end of the six days in the No. 3 compartment the water had not risen a quarter of an inch. I mention this matter to-day with reference to what Sir Nathaniel Barnaby mentioned, about giving commanders of merchant ships thorough confidence in these bulkheads, and so that it may go forth from this Institution that there are such ships afloat. I would also like to say that I am afraid Sir Nathaniel Barnaby will not get that confidence to which he has alluded, and for this reason. In all cases of loss of ships at sea it has not been the failure of the bulkhead through the admission of water, but it has been the failure of the bulkheads when the water got at the cargo which was in the hold. I have seen one case myself where the decks of the ship were forced up about nine inches and about 300 tons of jute was so pressed in the compartment after the ship had been saved, that it literally had to be teased out down almost to the very keel. In the light of recent events, too, there is no doubt about one thing, that the bulkheads of the ships have not given way in most cases where the steamers were outward bound from this country, but the steamers were lost from other circumstances, and one of the most dangerous circumstances of all is the case that has recently happened; that is, where steamers are struck in their bunkers. We all know that the water-tight doors of these compartments are absolutely compelled to be kept open, no matter how stringent the regulations are. If they are not kept open owners will have to maintain a much larger staff of men on board their ships, and that in these times certainly will not do. But it has occurred to me since this Paper was commenced, that as soon as the accident occurs, if the ship is struck in the bunkers, immediate attempts are made to close these doors. I may mention it was done in a recent case, and then as soon as the doors came to be about half shut, the inrush of water chokes

the door with coal and such like, and it was impossible to close it further. I know myself that the chief engineer in one case stood by up to the neck endeavouring to close that door ; he got it down about three feet, but the remaining two feet it was impossible to move it. It has occurred to me that what we require for these doors, in coal bunkers especially, is a sort of inside portable flap, and I throw out that suggestion to members to-day, that is to put a plate about three feet wide over the door, and two cast plates at the sides of the door, so arranged that when you come to close the water-tight door itself, these places can be supported, so to speak, the hood dropped down, and the two sides float in as the water and coal rush to the door. It may not be thoroughly effective, but it will enable men to clear the coal away and to close the proper door.

Sir Nathaniel Barnaby : My Lord and Gentlemen, I may be allowed to say that the safeguard which Mr. Maginnis has spoken of, is a safeguard which is absolutely indispensable for security, and is always fitted in the Queen's ships.

Mr. T. C. Read : My Lord and Gentlemen, I think that Mr. Martell has completely answered the queries of Sir Nathaniel Barnaby and it is hardly necessary for me to refer to them again. With reference to the point Mr. Denny raised, as to the arrangement of the seams of the plating it would appear that while the total stress is a maximum in the vertical direction near the middle line, yet on the other hand the plating would there receive greater support in this direction from the closer spacing of the vertical stiffeners which are only 30 inches apart, while the horizontal stiffeners may be 48 inches apart. When this is allowed for, there would probably not be much difference in the maximum stress on the rivets, whether the seams were arranged horizontally or vertically. Mr. Maginnis has spoken of cases where bulkheads were known to be successful, and in fact he quoted a case with regard to a bulkhead almost exactly similar to the case I have calculated, the bulkhead he referred to being 39 feet broad, whereas I have taken a breadth of 40 feet. It is extremely satisfactory to find that these tests have been made, and that the bulkheads have proved strong enough and thus, to some extent, the results of these calculations verified.

The President : I have now to thank Mr. Read for his valuable Paper, and I hope that he is gratified with the discussion which has ensued upon it, which, although short, has been a most exhaustive one.

CONCLUDING REMARKS.

Admiral Sir JOHN HAY: Gentlemen, at the conclusion of the four most interesting days which have occupied our time, I am sure you will permit me to be your spokesman in a duty which must devolve upon some one of us, and which, I trust, will not be insufficiently performed by me, and I only wish that I had the eloquence to express your views as you would like to have them expressed. In the early days of the Institution, and it is now a good while ago, we were fortunate in having as our President the late Sir John Pakington, and as Lord Hampton we grieved his loss excessively, for while he presided over us he made us the valuable Institution which we are, with a European and, I may say, with a world-wide reputation; but when, in the course of time, Lord Hampton was no longer here to preside over us, a happy inspiration seized upon some of the Members of the Council, and we were able to direct the attention of the Institution to Lord Ravensworth, who has so well filled the chair on this occasion. To those who are here to hear the interesting Papers, and even to those who have prepared those Papers, the meeting is sure of bringing together many friends and giving opportunities for discussion on interesting scientific matters, which send us away better informed and more ready to perform our duty in improving the architecture and other interests which this Institution is bound to attend to, but to the Chairman, although our President, the duty is a laborious one. I have no doubt Lord Ravensworth will acknowledge it is a labour of love, but the sedulous attention which he has shown and always does show to all the interests of the Institution in the chair and elsewhere must cause him, if not anxiety, at least some wear and tear; and it is the duty of the Chairman, without pretending to understand all the scientific matters which come before us, to obtain a general knowledge of the papers which are about to be read, and not only a general knowledge of the papers which are about to be read, but also of those gentlemen on whom he may call to add int rest to the discussion, and who will devote their time to studying the Papers and to making the matter interesting to the general public; and that duty is one which requires great care and attention, a care and attention which I am sure Lord Ravensworth has always shown. (Hear, hear.) When the attention of the Council was directed to Lord Ravensworth, we knew well that for many years he had shown independence of character and a knowledge of the commercial and naval interests of this country as a member of the House of Commons, which pointed to him as a very fit and proper person to preside over us; but it was not until we tried him in the chair that we were quite sure how well he would fulfil the duties that have devolved upon him. As years go on I think we shall even more highly appreciate his character, but at this moment we have had sufficient experience of him to enable me now to invite you, as I am sure you most cordially will, to give your thanks to Lord Ravensworth for having presided over us this year.

Sir NATHANIEL BARNABY: Gentlemen, I have much pleasure in seconding the proposition which has been put before you by Sir John Hay, and, speaking as a very old member of this Institution I may say, and I know it will please Lord Ravensworth to feel that there is truth in it, that we have never had a better series of meetings than we have had this year. The Papers have been full of interest and full also of novelty, and while we shipbuilders cannot help feeling that the art we profess is no longer a

mystery, we feel that in this great home of research that has been established, we may claim that we are placing on a very strong basis the true science of shipbuilding. The science of shipbuilding advances so rapidly, that I may say confidently, that if the drawings of a ship that was designed by me two years ago, or by my friend Mr. White two years ago, were placed in the hands of a thoroughly capable naval architect to-day, he would say, these fellows are behind the time—we cannot have that. Two years are quite sufficient to put out of date a design for a ship of war. While things are going on so rapidly, it is of the greatest value to all of us that we should get contributions, as we do, from our generous friends in France, from our friends away in the north (Mr. Denny and his firm for example), who come down here, and those who come from all parts of Great Britain, helping each other to attack the great problems which crowd upon us, and make us feel that the older we are, the more we have to learn. I very heartily second the vote of thanks which has been proposed in such a proper way by Sir John Hay—Thanks to our noble Chairman. (Carried by acclamation.)

The PRESIDENT: Gentlemen, it is difficult always to convey one's thoughts when they are disturbed by outward sounds or manifestations such as you have honoured me with just now, but it is especially difficult for a man in my position to rise and thank a learned assembly for their appreciation of the humble services performed by an entirely unscientific person who occupies the chair in such a hall as this, because I am always oppressed by the feeling that I am not able to do the duty in the way that you would wish it done, and that I, above all, should wish to see it done. But, Gentlemen, at the same time let me say if I do my best I can do no more. Now, Gentlemen, there are two or three remarks that I should like to make to you, because they are of a practical nature, and have reference specially to our proceedings in future years. In the first place, allow me to congratulate you all upon the programme that has been gone through this year. It proves, I think, unmistakably, not only to members of the Institution, but to the outside world, which is the important thing, because we have organs of communication which circulate the proceedings of this Institution for the benefit of the outside world—that our members are not only anxious, but are perfectly competent to keep abreast with the times, and the requirements of the times in regard to both the two great studies of Naval Architecture and Marine Engineering. I think that our proceedings this year have been of remarkable interest—they have dealt with new problems, they have dealt with new requirements, and they all point in the direction of progress; and, depend upon it, that if we are to keep abreast with the rest of the world, we must keep as much as we possibly can at the head of those two great studies. None of us can say that that is always possible, nor that we shall not sometimes be liable to be outstripped by able competitors, but if we do our best, in all probability the old Anglo-Saxon energy will enable us to keep pretty well to the front. Gentlemen, we have departed from our usual rule of proceeding this year in holding a morning sitting on Saturday. I will remind you that had we not come to that conclusion—that is the Council—we should have lost two Papers of extreme value—I venture to say the two Papers which we have had this morning read to us—one of extreme importance to naval officers, the other of equal importance to the Merchant Service. I would refer for a moment to the Paper upon the turning of ships; and the extraordinary importance that seems to me to attach to that Paper is, that it brings out the fact that

our naval officers are studying this question most zealously, in view, probably—and I speak in the presence of eminent naval officers—of the altered conditions of naval tactics in the future. With regard to bulkheads, which was the subject of the second Paper, recent occurrences have directed public attention to this subject, and a Paper such as we have had on the strength of bulkheads is particularly opportune, proceeding from such a source as this Institution. I venture to hope that a morning sitting on the Saturday will not be drawn into a precedent, because, after all, human nature is human nature, and even eminent men and hard heads are subject to exhaustion, and I think three days of the extremely abstruse and scientific discussions which are carried on in this hall are enough for most people. Therefore I say, Gentlemen, I hope it will not be drawn into a precedent. The small attendance this morning I believe to be rather an accidental circumstance. Perhaps you are not all aware that there is a very great, I will call it function, going on—a most important event in the history of this country, the opening of the Tilbury Docks, and no doubt many of our eminent members are attending that most interesting ceremony. I would remind you in reference to our proceedings, that I believe we are the only learned society in this metropolis that can boast of reading eighteen Papers of a highly scientific character at any one of their annual meetings. I think it is due to the Institution that that should be recognized. (Hear, hear.) Now, Gentlemen, I want to make one suggestion, and I hope it will meet with your approval. I will ask members to turn their attention to it, and act upon it if they are good enough to approve of the suggestion, and that is, that Papers should be sent in fourteen days at least before the Council Meeting which always precedes the annual assembly here. I will point out in two words, Gentlemen, what I consider the importance of that suggestion, and I think it will be borne out by our most eminent members. The subject matter of many of these Papers is of so abstruse a character, that unless members have had those Papers in their hands before, and for some little time before, to prepare themselves for fair criticism, it is almost impossible coming into this room to take that part which they wish to take, unless they have had the opportunity of carefully studying them. I will illustrate that position by reference only to two Papers, one the Paper of Mr. Froude—and when I speak of Mr. Froude's Paper, I must remind you it is the result, the praiseworthy result, on his part of a long succession of investigations of the most careful and the most elaborate kind, and which explain the length of the Paper to which it was my duty to allude a day or two ago. It is almost impossible to give the result of the amount of labour which he bestows upon the subject, except at very considerable length, but I appeal to the scientific members who are present, whether that is a Paper which you can on the spur of the moment attempt to analyse and adequately discuss. Then we had a Paper this morning from Mr. Read of a most valuable description. I have not the least doubt whatever that that Paper also requires very careful and somewhat lengthy consideration thoroughly and adequately to discuss it, and I offer this suggestion both to writers of Papers and to members of the Institution who wish to take part in the discussions arising on the Papers. Well, now, Gentlemen, I may congratulate you upon really a more material matter, and that is the enormous increase of our numbers this year. They amount to no less than sixty-one new members and associates, by far the largest number that has ever joined the Institution in any one year. That shows clearly that the merits of this Institution are thoroughly appreciated out of doors, and that our influence happily is growing in the country. We have agreed, and the members have confirmed the

Council's opinion upon the subject, to hold an autumn gathering on the Mersey this year at Liverpool. I have not the least doubt that we shall be received, as everybody is received—and I speak from experience—in Liverpool, with all the courtesy, hospitality and generosity which marks that great city, and we shall also in all probability add to our numbers by meeting there. I think there are no other matters to which I need at present allude. I offer that suggestion, and I hope it will be acted on, for I am quite sure it is to the interest of the Institution that it should be. I thank you, Gentlemen, with my whole heart for your extremely kind reception of the proposal which my friends Sir John Hay and Sir Nathaniel Barnaby have made to you. I might just be allowed to say before I sit down that I think the thanks of this Institution are very greatly due to Mr. Martell and his colleagues at Lloyd's, for the number of recruits to our ranks, which their efforts have brought. I am able to tell you that they brought no less than thirty-five out of sixty-one new members and associates by their energy and strong feeling for the welfare of this Institution. I may also tell you that Mr. Watts, whose presence we have missed this year, who is now at Elswick, has brought no less than twelve recruits from my own country, and I may tell you that our branch Institution, which I am happy to say is in association with this great Central Institution, is doing admirable work—they are reading most valuable Papers, and it is a very thriving Institution, also largely adding to its numbers in that great commercial port and neighbourhood. I think it will be satisfactory to you to hear that we also owe Mr. Watts our thanks for bringing us those twelve recruits from the North of England. I will not detain you longer, Gentlemen, but I thank you with all my heart, and as long as you are kind enough to keep me in this chair, my utmost energies will be devoted, as far as lies in my humble power, to promoting the welfare of this valuable National Institution.

Admiral the Hon. E. R. Fremantle: My Lord, I am a comparative stranger to this hall, although not a stranger to the merits of this Institution, but though the proverb which we often hear is perfectly true about bad workmen complaining of their tools, I am quite sure it is equally true, that no workman, whether he be good or bad, can work unless his tools are suitable. I think, therefore, you will all agree with me, and I think, my Lord, you especially will agree with me, that it is our duty before we leave this hall to return our thanks to the Members of the Council who have given us this extremely valuable and varied programme of lectures. I say they have given them us, because I have every reason to believe they have induced many of the lecturers who have given us their views to appear before us. It is quite unnecessary for me to detain you long to prove that "good wine needs no bush," and therefore I will only just add this, that many of the Members of this Council have names which are household words, not only in this country, but throughout the world where maritime interests are considered and appreciated. I beg, my Lord, to move that the thanks of this meeting be given to the Members of the Council for their assistance to you.

Mr. J. Ward: My Lord, I rise with very great pleasure to second the vote of thanks so well proposed by Admiral Fremantle to the Members of the Council of this Institution. He has said very truly that their numbers include names which, in their own profession, are as familiar as household words, not only in this country but on the Continent. The Members of the Council, if I may be allowed a shipbuilder's expression, are indeed worthy trailboards to the very vigorous figure-head this

Institution has in yourself, my Lord, and they show how real their interest is in the Institution, when your Lordship can say that two out of their number have this year brought forward five-sixths of the large addition made to the membership. I am quite sure you will all join with me in according to the Council the vote of thanks which Admiral Fremantle has asked of you. (Carried unanimously.)

Mr. B. Martell: My Lord, on the part of the Members of the Council of this Institution, I have to return our very sincere thanks to Admiral Fremantle for the very appreciative remarks which he has made in proposing this vote of thanks, and to Mr. Ward in seconding it. I can only say that the desire of every Member of this Council is to increase the usefulness of this Institution, and beyond that it is a very great source of gratification to me as an individual member of the Council to feel how harmoniously we all work together, and as has been very properly said in one of the scientific journals which I read to-day, the *esprit de corps* among us is very gratifying when we compare our Institution with many of the other scientific Institutions. The interest we take will, I am sure, be continued, and the Members of the Council of the Institution, as far as they can, will always exert their influence and give their time to advance the interests of this Institution. I can only very cordially on our part express our thanks to Admiral Fremantle and to Mr. Ward.

Mr. W. Denny: My Lord, I rise to propose a vote of thanks to the Society of Arts for the use they have so kindly permitted us to make of this hall. I think every one of us who has taken part in these meetings must feel that it would be very difficult to get any place where we would be as comfortable and as well able to carry on our meetings as we are here. We have, however, even more for which to thank the Society of Arts than their loan of this hall for these present meetings. When Sir Nathaniel Barnaby was speaking of the Institution, he very happily termed it a home of research, and he did so with good reason I think, for we have not only here that *esprit de corps* to which Mr. Martell referred, but we have also the feeling of inhabiting a home, and it is entirely due to the Society of Arts that we possess such a feeling. We have met here year after year so frequently and continuously, that there is hardly one of us who has taken part in these meetings for many years past but has come to feel that this hall is his professional home. I hope the Society of Arts will not think that in thus speaking we are presuming on their kindness or intending to take possession. We only desire to recognize the feeling they have conferred on us by their continuous kindness. Any one who has entered this hall as I did, a youngster, who has acquired years and experience in it, who has given and taken blows in it, and felt the delight of life in its contests, a life consisting in something more than patting each other on the back, must have an affection for it. I am sure, any way, Mr. Martell has. We must feel that in this hall, with its colossal pictures offering an alternative of interest and charm to occasional dull Papers, with the associations which have clustered about it, the Society of Arts has given us something far more than any mere loan for a year could convey. Therefore, in returning thanks to that Society we should do so not only for this year, but for a past record of years that have added a charm to the Institution over which you, my Lord, so ably preside.

The President: Gentlemen, I am going to ask you to allow me most cordially to second the motion of my friend Mr. Denny, and to entirely endorse all he has said. We do indeed owe a very

great debt of gratitude to the Society of Arts. We all know the extreme difficulty that we should be placed in were they to withhold from us their kind permission to use this beautiful hall. In a great city like this, the command of a hall implies a very considerable amount of expense. They most kindly save us the whole of that expense, and besides that I do not think we could possibly find any place that would be so suited to our usual numbers and to our proceedings here than this arena. I will only sit down expressing the hope *dum nos bene gerimus*—may the dreadful word "eviction" never be uttered in reference to ourselves by that excellent Society of Arts. I would venture now to ask you to join with me in a vote of thanks, which is as much and even more deserved than any that have preceded it, and that is to no less a person than our Secretary. (Hear, hear.) Now, Gentlemen, I am in a position to bear ample and most thorough testimony to the work done by our Secretary. I probably have as much communication with him in the course of the year, and probably even more than any other member. He shows a readiness, and not only that, Gentlemen, but an ability in discharge of his very often difficult duties, which I think it would be almost impossible to equal. We know very well his distinguished career in early life; we know how thoroughly he seems to be able to place himself abreast of our proceedings; we know his courtesy and thorough integrity, and also that his services are always at the disposal of any member or associate of this Institution. It is ill praising a man to his face, but I think it is due to our Secretary to point out some of the qualities and acquirements which he places so readily at the disposal of this Institution. I may in conclusion mention this, that the work of the Secretary at this time is extremely heavy every year. He is called upon not unfrequently to translate elaborate scientific Papers from foreign languages into English. That is no light task to impose, and it is not every secretary that could do that at all. I think it is right just to mention that circumstance, because we are always most grateful to our foreign contributors, and acknowledge to the full the value of their communications, but I am afraid that they would not be so useful if they were read in their original languages from this desk as we should wish them to be. Therefore I think upon that score our Secretary deserves special credit, and I am quite sure you will unanimously join with me in according a vote of thanks to the Secretary, and also our gratitude to him for his services. (Carried unanimously.)

Mr. G. Holmes (the Secretary): My Lord and Gentlemen, I would only say that I am extremely obliged to you for your kindness, which altogether exceeds my deserts.

The President: Gentlemen, I have nothing further to do than to say may our next merry meeting be as useful and as happy a one as that which has just passed.

LLOYD'S REGISTER OF BRITISH AND FOREIGN SHIPPING.

REPORT

ON THE EFFECT OF PUNCHING, DRILLING, AND RIMING MILD STEEL PLATES.

(Referred to by Mr. William Parker. *See Page* 134.*)*

In accordance with the Committee's instructions we proceeded last month to Sheffield, for the purpose of carrying out a series of tests at the works of Messrs. Charles Cammell & Co., on the properties of mild steel under the various conditions of punching, drilling, &c., and with plates both annealed and unannealed after punching. After witnessing these tests, and visiting the works of Messrs. John Brown & Co., we proceeded to Liverpool for the purpose of superintending the punching, &c., of another still more extensive series of steel plates furnished by Messrs. John Brown & Co., and by the Bolton Iron and Steel Company, and a series of Best Best iron boiler plates obtained from Messrs. Seaward & Co., of London. These specimens, after having been punched, &c., according to our instructions, by Messrs. James Jack & Co., of Liverpool, were forwarded to the Chain and Anchor Testing Works at Saltney, and we subsequently witnessed the series of experiments made there upon them.

The results of all the experiments above alluded to are appended, and the following remarks thereon are respectfully submitted for the consideration of the Committee.

The objects of the experiments, briefly stated, were to ascertain approximately to what comparative extent mild steel plates and iron plates are injured when they are punched; how this injury is affected by variations in the thickness of the plates, or variations in the relative sizes of punch and die; and also how the injury produced by punching is affected by subsequent riming or annealing. It has been shown on several occasions and on high authority that considerable loss does arise from punching steel plates when they are not subsequently annealed; and it has also been shown from experiments that the loss is affected considerably by the kind of punching applied, the open die or conical punching being asserted to cause less loss than by punching a nearly parallel hole with a close die.

The experiments show that thin steel plates lose comparatively little in punching, being for the ¼-in. plate only 8 per cent., for the $\frac{6}{16}$in. plate 18 per cent., and for the ½-in. plate 26 per cent., while in the thicker plates of ⅝in. and ¾in. it rises so high in some cases as 33 per cent., or one-third of the strength of the plate. The iron plates which were about ¾in. thick lost from 18 to 23 per cent. by punching.

At Messrs. Cammell's works, we tried the effect of changing the relative sizes of the punch and die, the results being given in Table I. and II., where the pieces tested were all from the same plate. The punched specimen in Table I. had a taper of only $\frac{1}{16}$ of an inch or $\frac{1}{32}$in. each side of the hole, and broke at 22·9 ton per inch, showing a loss of 17·8 per cent., while with a taper of ¼ of an inch, or ⅛in. each side of the hole; the tensile strain was (*see* Table II.) 24·6 tons per inch, showing a loss of only 12·3 per cent. Going to a still higher taper, viz., ⅛ of an inch each side, the hole became rough and unfair, and the tensile strain became reduced, viz., 21·04 tons per inch, showing a loss of 21½ per cent. In the specimens tested at Saltney no such gain from taper punching was observable, but it should be stated that in none of those specimens did the amount of taper correspond exactly with that of specimen No. 1, Table II., which gave such good results at Messrs. Cammell's. The taper had to be made somewhat less to suit the dies available at Messrs. Jack's establishment at Liverpool, where the specimens were punched, as the dies next in size would have given too much taper and produced an unfair hole like specimen No. 2, Table II. The effect of punching in the thicker specimens of steel was not only to cause them to break at a less tensile strain, but it also prevented the specimens from stretching, causing them in fact to break with a crystalline fracture. This at first sight would seem to indicate that the quality of material had been deteriorated from hole to hole. It is not so, however, as the present experiments show, and in this they confirm results previously obtained in this country and by the French Naval Authorities with material of a similar description. The experiments show that the injury is confined to a zone of metal round the hole less than ⅛ of an inch thick; for when ⅞in. holes were punched in ¾in. plates and then rimed out to 1⅛in. diameter the material stood practically as high a tensile strain per square inch as the solid plate or the drilled plate had done.

This is an important practical result, and one that has excited attention, and many attempts have been made to account for this change in the condition of so narrow a zone round the hole producing so much effect on the remainder of the section as to cause it to break with a crystalline fracture and without appreciable stretching. It has been shown by very careful experiments in France that in the process of punching both steel and iron plates, the temper of the material becomes altered for a short distance round the hole, the metal becoming harder and less capable of stretching. Owing to its hardness and inability to stretch, this part round the holes, when the piece is under tension, takes a higher proportion of the strain than the other more yielding parts, and hence it reaches the breaking strain sooner. When the part round the hole gives way its strain is transferred to the other parts, and at first the extra strain falls suddenly on the adjoining parts, and this intense local strain, so far as the experiments would seem to indicate, sets up a tearing action across the remaining section of the plate. It is important that inequalities of strain of this kind should be reduced so far as possible in

practice, and the subject has occupied our careful attention. It has been shown that in thin steel plates the loss from punching is comparatively small as compared with iron. In thicker plates, however, say beyond half an inch thick, the loss is greater than in iron, and means should be adopted for reducing the loss by punching as far as may be practicable, especially where it is subject to high tensile strains. It has been mentioned that riming $\frac{1}{8}$ of an inch round the hole after punching fulfils such a requirement entirely. Countersinking would also produce this effect to a certain extent. Another process is to anneal the plates after punching. In the specimens tested by us where this was done the punched and annealed plates stood as high and sometimes a higher tensile strain per square inch than the solid plate, or than the drilled plate. Either punching the holes small and then riming them, or drilling the holes entirely, or annealing the plates after ordinary punching might therefore be resorted to for keeping up the continuity of the strength, according to which would be most convenient for the particular work in hand.

It is not improbable that by the introduction of an improved punch and die the loss of strength in thick plates may be considerably reduced, and special patent punches are being tried at the present time with a view to this object.

A specimen was tested also to ascertain whether it sustained a loss of strength by being hammered flat on an anvil, and no deterioration of strength was found to arise therefrom.

Having consulted Messrs. Laird Brothers, of Birkenhead, in reference to their recent experience in the construction of two steel vessels, we were shown the results of a number of experiments made by them to test the qualities of the plates and butt straps, both when punched and unpunched, and the results of some of those experiments have kindly been furnished for the information of the Committee, and are herewith appended. They found the strength and ductility of the steel plates, both punched and unpunched, so far superior to iron that no extra precautions were necessary, either in annealing the plates after punching or in riming the holes, and this applied to plates up to $\frac{8}{16}$ in. thick. Their experience had not extended beyond this point. In the vessels built by them steel rivets had been used successfully, and every endeavour to detect bad or untrustworthy rivets had failed, but it was stated that the steel employed for rivets was of a special quality of great ductility.

It is respectfully submitted that the experiments witnessed by us appear to bear out the conclusion arrived at by Messrs. Laird Brothers, that in small vessels and up to plates of $\frac{8}{16}$ in. thick, no special precautions are necessary for steel vessels more than are required for iron ships, while they confirm the practice of the Committee in requiring the sheerstrake, deck stringer plates, and garboard strakes to be annealed after punching when they are above $\frac{8}{16}$ in. thick. The experiments also suggested in very large ships the necessity of annealing such other parts of the vessel as are subject to severe tensile strains, or of riming the holes, or adopting other means for maintaining the strength of the butt connections.

These experiments also show that in case of marine boilers the shell plates should be drilled in place, or if punched they should subsequently be annealed, as has been required in the steel boilers which have already received the sanction of the Committee, and are now in course of construction.

WILLIAM PARKER,
Chief Engineer Surveyor.

WILLIAM JOHN,
Assistant Chief Surveyor.

20th March, 1878.

LLOYD'S REGISTER OF BRITISH AND FOREIGN SHIPPING,
2, WHITE LION COURT, CORNHILL, E.C.

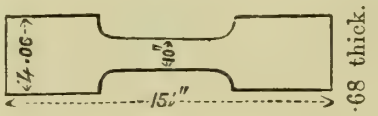
EXPERIMENTS ON THE EFFECT OF PUNCHING STEEL AND IRON PLATES.

TABLE I.

First series made on Messrs. C. Cammell & Co.'s subcarburized steel as supplied for boiler-making purposes at their establishment, Sheffield, 18th February, 1878.

New No.	Old No.	Thickness of Plates.	Description.	Size of Holes.	Breaking strain in tons per square in.	Per centage of loss from strength of plain plate.	Remarks.
0	1	·675	plain . . .	...	...	...	Broke through attachment (useless).
1	1*	·68	plain . . .	...	27·05	...	This piece was cut from first specimen & was only 1 in. sq. (A)
2	3	·687	punched . .	$1\frac{1}{8}$...$1\frac{3}{16}$	22·92	17·8	Silky fracture except near holes.
3	4	·712	punched and rimed.	$\frac{7}{8}$...$1\frac{1}{8}$	28·81	...	Silky fracture.
4	5	·7	punched & annealed	$1\frac{1}{8}$	28·98	...	Do.
5	2	·675	drilled . . .	$1\frac{1}{8}$	28·75	...	Do

Strength of plate taken to be the mean of the plain and the drilled specimen, viz., 27·89 tons.



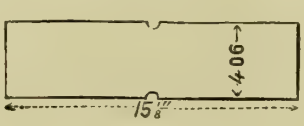


TABLE II.

Second series made on Messrs. C. Cammell & Co.'s subcarburized steel for boiler-making at their establishment, Sheffield, 19th February, 1878, to show the effect of a large proportion of die to punch.

New No.	Old No.	Thickness of Plates.	Description.	Size of Holes.	Breaking strain in tons per square in.	Per centage of loss from strength of solid plate.	Remarks.
1	1	·7	punched . .	1·1...1·35	24·46	12·3	Silky fracture.
2	2	·7	punched * . .	1...1·35	21·04	24·5	Do.
3	3	·7	punched and rimed.	⅞...1·15	25·69	7·9	Do.

Strength of Plate taken from previous experiment, viz., 27·89 tons.

* This appeared to be too much taper to make good punching and a clean hole.

·7 thick.

TABLE III.

Third series made by Messrs. C Cammell & Co., Sheffield, on their Best Best iron plates, such as they supplied to the Admiralty.

New No.	Old No.	Thickness of Plates.	Description.	Size of Holes.	Breaking strain in tons per square in.	Per centage of loss from strength of plain plate.	Remarks.
1	1	·74	plain . . .	...	24·88	...	Mostly crystalline.
2	2	·74	plain . . .	...	22·53	...	Do.
3	3	·74	plain . . .	...	22·56	...	Do.
4	4	·74	punched . .	1·1...1·17	18·19	18·8	Slightly crystalline.
5	5	·74	punched . .	1·1...1·32	18·93	8·8	Do.
6	6	·74	punched and rimed.	⅞...1·13	21·17	9·2	Do.
7	7	·74	drilled . . .	1·13	22·57	3·	

Strength of plate taken to be the mean of three plain specimens, viz., 23·32 tons.

·74 thick.

TABLE IV.

First series made at Saltney, 23rd February, 1878, on steel plates manufactured by the Bolton Iron and Steel Company, for boiler-making purposes.

New No.	Old No.	Thickness of Plates.	Description.	Size of Holes.	Breaking strain in tons per square in.	Per centage of loss from strength of solid plate.	Remarks.
1	2	·75	plain . . .	...	26·4	...	Silky fracture.
2	3	·75	punched with close die . . .	1·09 1·18	20·	24·2	Uniform crystalline fracture.
3	4	·75	punched with open die . . .	1·08 1·24	18·69	28·7	Do.
4	1	·75	punched and rimed.	$\frac{7}{8} \times 1\frac{1}{8}$	28·2	...	Fine silky fracture, slightly crystalline towards edges.
5	5	·75	close punched and annealed . .	1·09 1·16	31·71	...	Silky fracture, slightly crystalline.

TABLE V.

Second series made at Saltney, 25th February, 1878, on steel plates manufactured by Messrs. John Brown and Co., Sheffield for boiler-making purposes.

New No.	Old No.	Thickness of Plates.	Description.	Size of Holes.	Breaking strain in tons per square inch.	Remarks.
1	18	·75	plain	...	...	Broke through pin hole (useless).
2	19	·75	punched with close die	1·09...1·19	20·5	
3	20	·75	punched with more open die . .	1·09...1·22	19·08	
4	21	·75	punched and rimed .	$\frac{7}{8}$...1·16	21·72	Broke through pin hole without signs of distress at rivet holes.
5	21	·75	punched and annealed .	1·09...1·18	28·58	

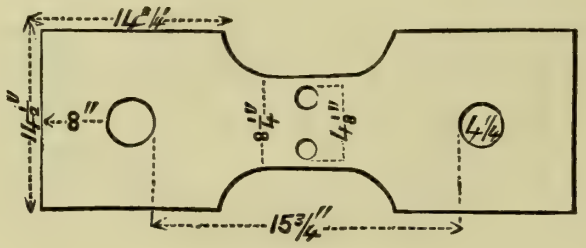


TABLE VI.

Third series made at Saltney, 23rd and 25th February, 1878, on Best Best iron boiler plates, selected from the stock of Messrs. Seaward & Co., London.

New No.	Old No.	Thickness of plates.	Description.	Size of Holes.	Breaking strains in tons per square in.	Percentage of loss from strength of solid plate.	Remarks.
1	23	·75	plain . . .	...	19·92	...	
2	7	·78	punched with open die	1·08 1·25	15·59	22	
3	8	·78	punched with close die	1·08 1·18	15·90	20	
4	9	·78	punched with close die	1·08 1·17	15·3	23	
5	24	·75	punched and rimed	1·16	17·6	11·4	
6	25	·75	punched and annealed	1·08 1·18	19·13	4·0	
7	6	·78	drilled . . .	1·14	19·11	4·0	Silky fracture, very slightly crystalline.

TABLE VII.

Fourth series made at Saltney, 23rd and 25th February, 1878, on steel plates manufactured by Messrs. John Brown & Co., Sheffield, for shipbuilding purposes.

New No.	Old No.	Thickness of Plates.	Description.	Size of Holes.	Breaking strain in tons per square in.	Per centage of loss from strength of solid plate.	Remarks.
1	11	·25	plain . . .	...	32·07	...	Silky fracture.
2	10	·25	punched with open die	·69...·74	29·48	8·1	Ditto.
3	13	·375	plain . . .	...	29·59	...	Ditto.
4	12	·375	punched . .	·84...·94	24·06	18·7	Ditto.
5	15	·484	plain . . .	...	28·98	...	Ditto.
6	14	·468	punched . .	·97...1·11	21·38	26·2	Crystalline fracture.
7	17	·593	plain . . .	...	29·55	...	Silky fracture.
8	16	·593	punched . .	·96...1·1	19·57	33·8	Crystalline fracture.

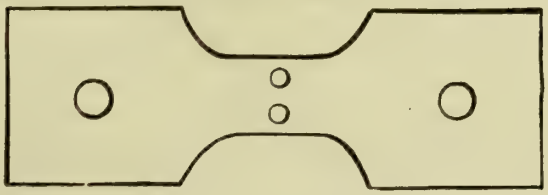


Table VIII.

Fourth series made at Messrs. C. Cammell & Co.'s Works, Sheffield, 18th February, 1878, on three specimens cut from a plate manufactured at the Motala Works, Sweden, to corroborate the results obtained from tests by the Motala Company.

No.	Thickness of plates.	Description.	Size of Holes.	Breaking strain in tons per square inch.	Remarks.
1	·45	plain	...	28·09	This piece was struck a few heavy blows on an anvil, bent slightly and then bent back again and struck flat. Hammering does not seem to affect it.
2	·45	plain	...	27·13	
3	·45	punched . . .	·875	28·22	

Nos. 1 and 2.

Table IX.

Series made by Messrs. Charles Cammell & Co., Sheffield, on their subcarburized steel for boiler making purposes.

No.	Thickness of plates.	Description.	Size of Holes.	Breaking strain in tons per square in.	Percentage of loss from strength of drilled plate.	Remarks.
1	·75	drilled . . .	1⅛"	29·27	..	Silky fracture.
2	·75	punched . . .	1⅛"	19·76	33	Crystalline fracture.
3	·75	punched . . .	1⅛"	19·84	33	Ditto.
4	·75	punched and annealed .	1⅛"	27·87		

NOMINAL INDEX.

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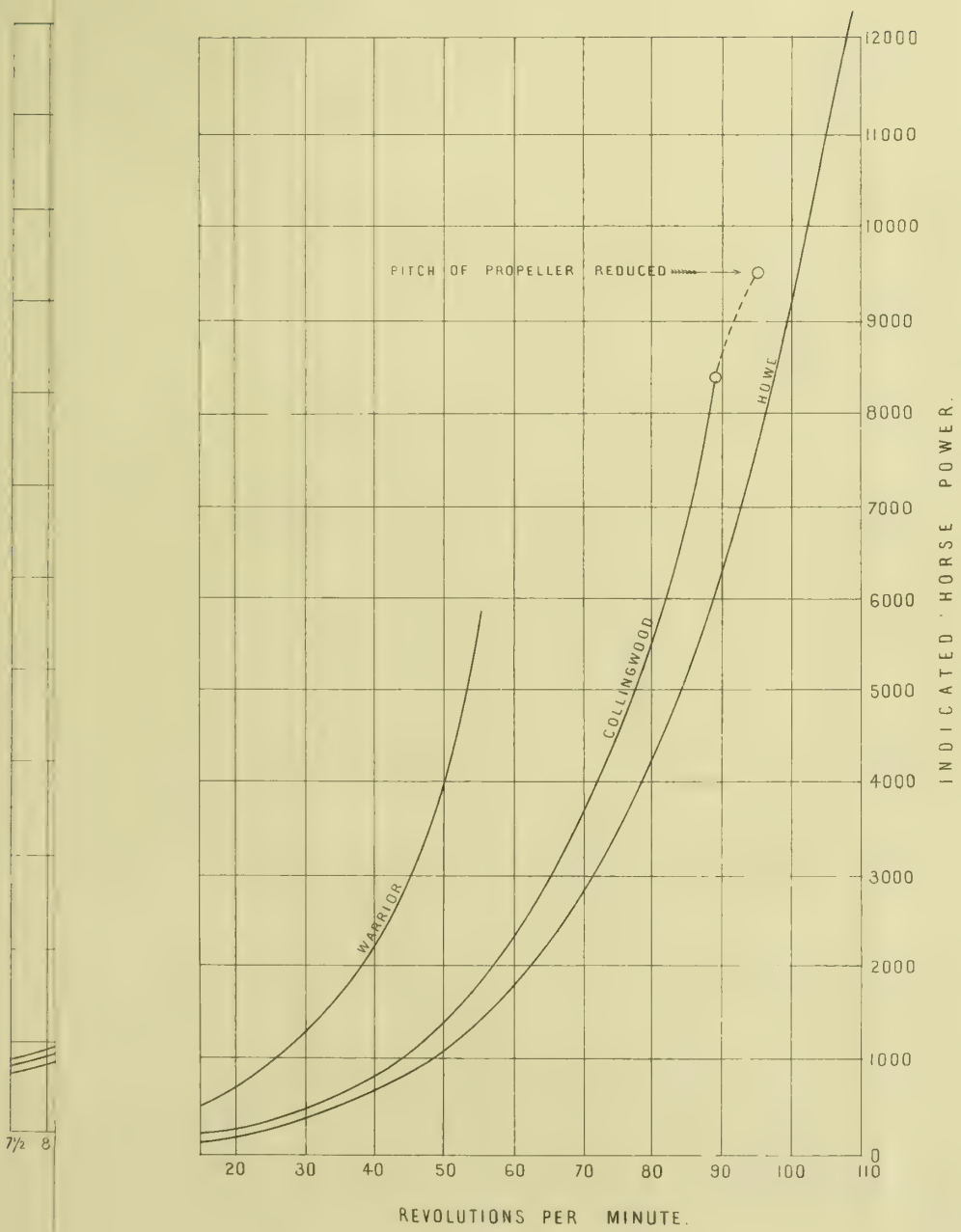
War Ships.

FIG. 4.



To Illustrate Mr. W. H. White's Paper on the Speed Trials of Recent War Ships.

FIG. 1.

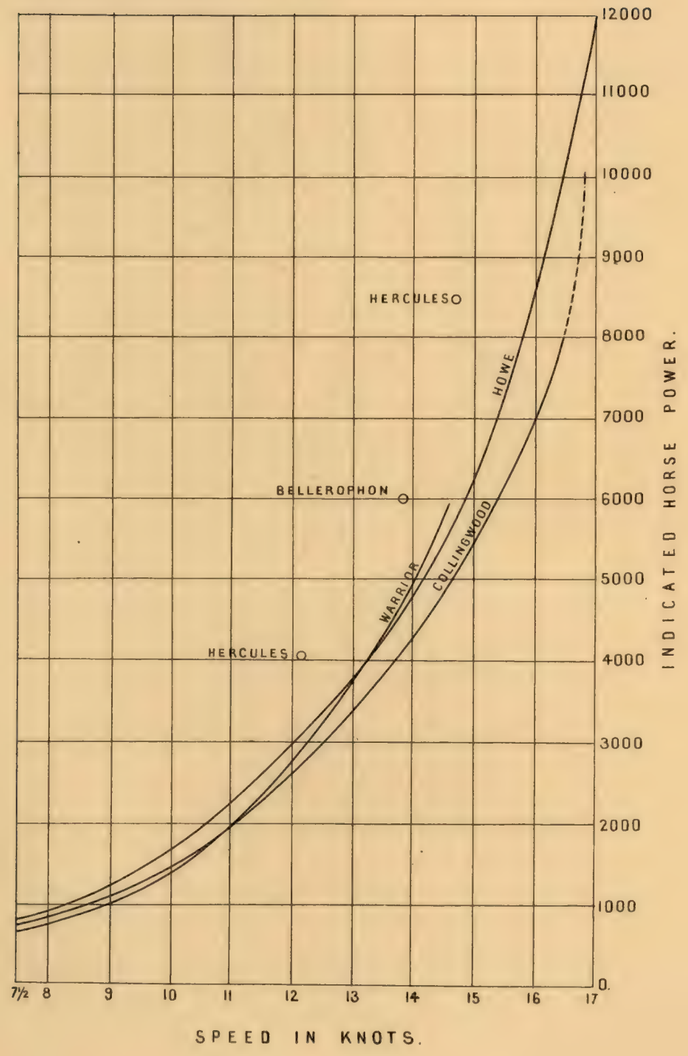


FIG. 2.

$$\frac{(\text{DISPLACEMENT})^{\frac{2}{3}} \times (\text{SPEED})^3}{\text{INDICATED HORSE POWER.}}$$

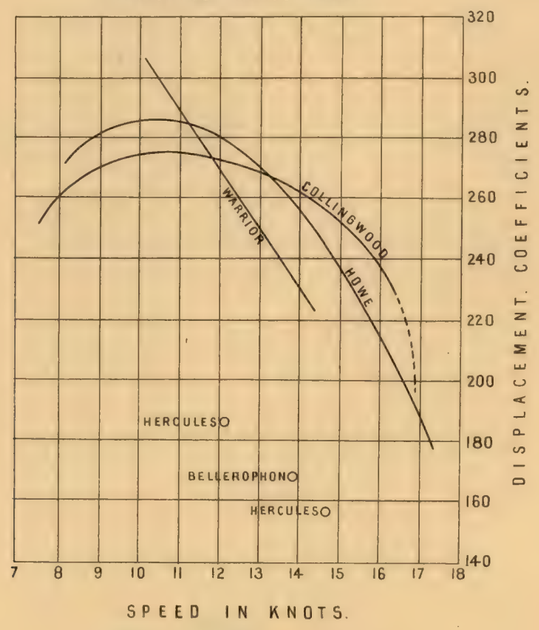


FIG. 3.

$$\frac{\text{AREA OF MIDSHIP SECTION} \times (\text{SPEED})^3}{\text{INDICATED HORSE POWER.}}$$

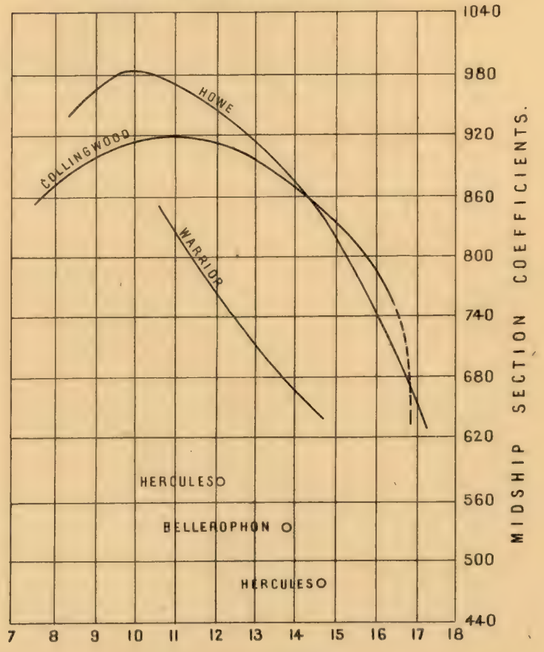
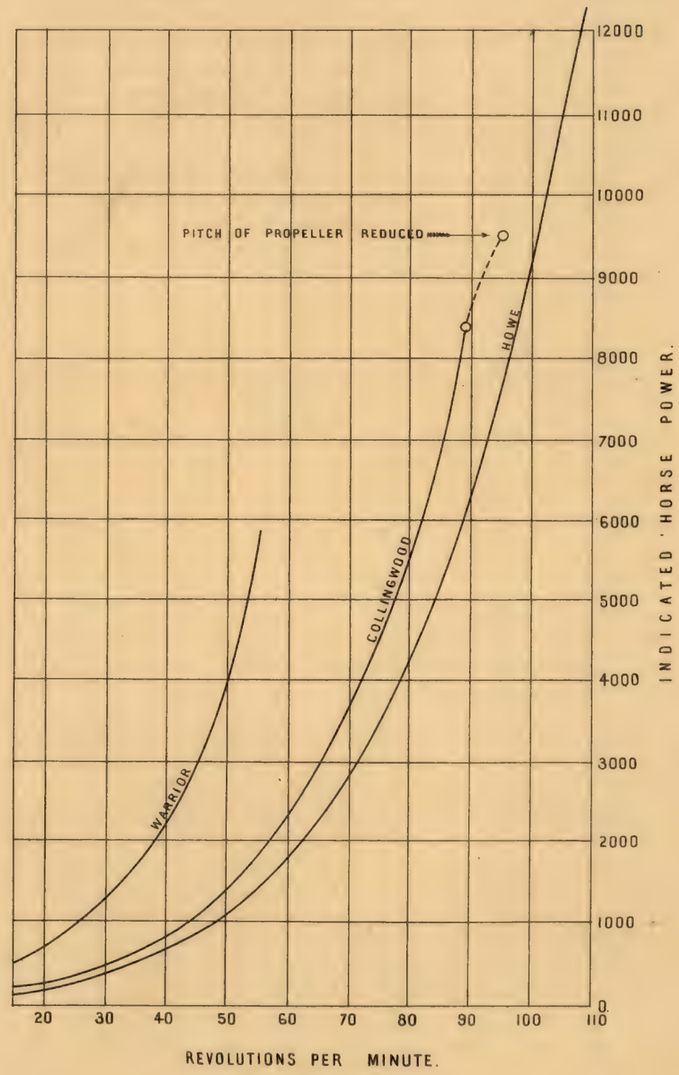
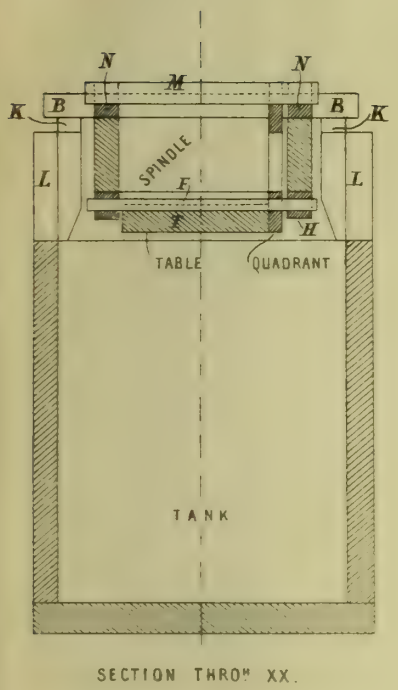


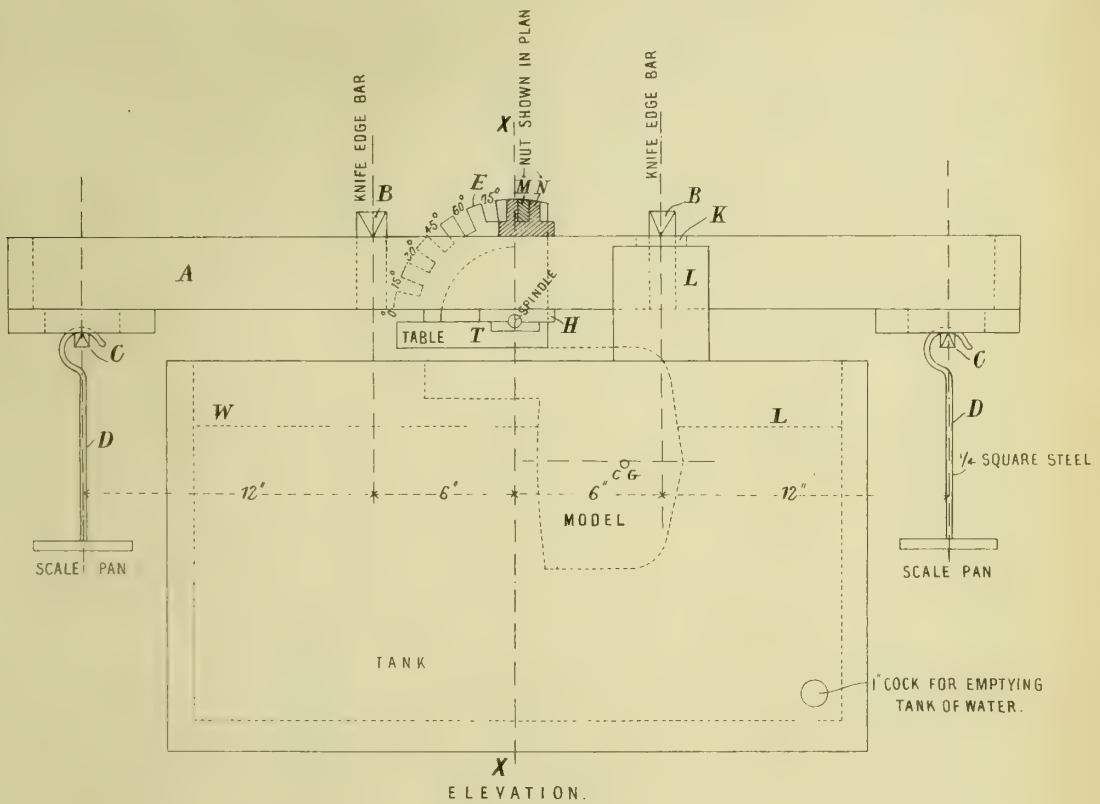
FIG. 4.



To Illustrate Mr. F. H. Heck's Paper on an Improved Mechanical Method of Finding the Stability of a Vessel.



SECTION THRO" XX.

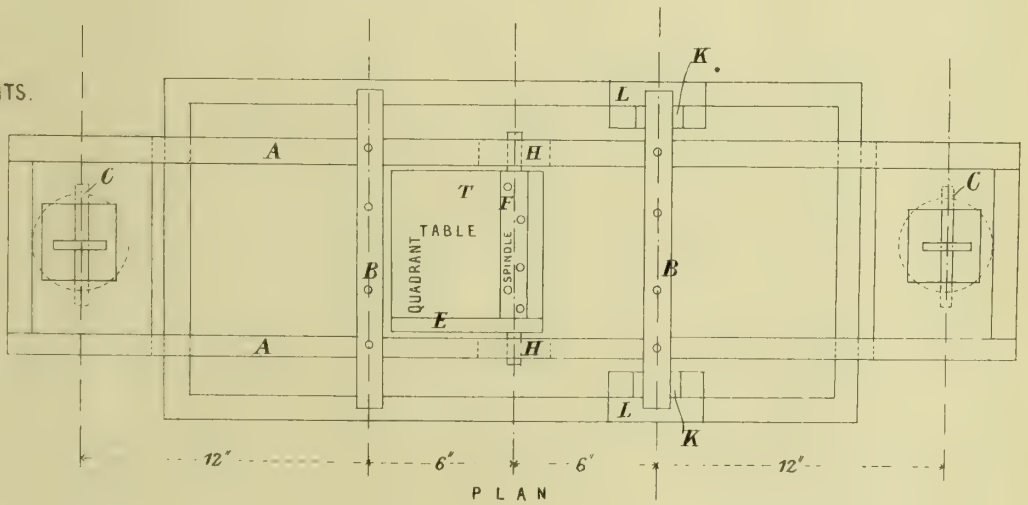


ELEVATION.

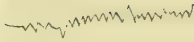
STABILITY BALANCE EMPLOYED IN EXPERIMENTS.

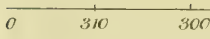
Scale 1½ in.=One Foot.

FIG. 1.



PLAN

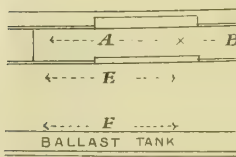




ICHING.

"

DEPTH 26ft.



N°2

To Illustrate Mr. J. H. Heck's Paper on an Improved Mechanical Method of Finding the Stability of a Vessel.

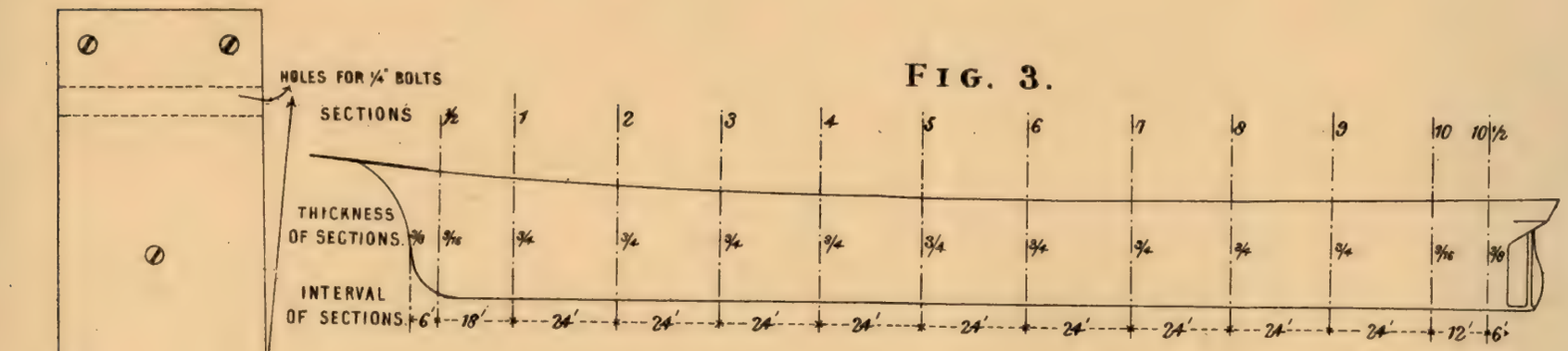
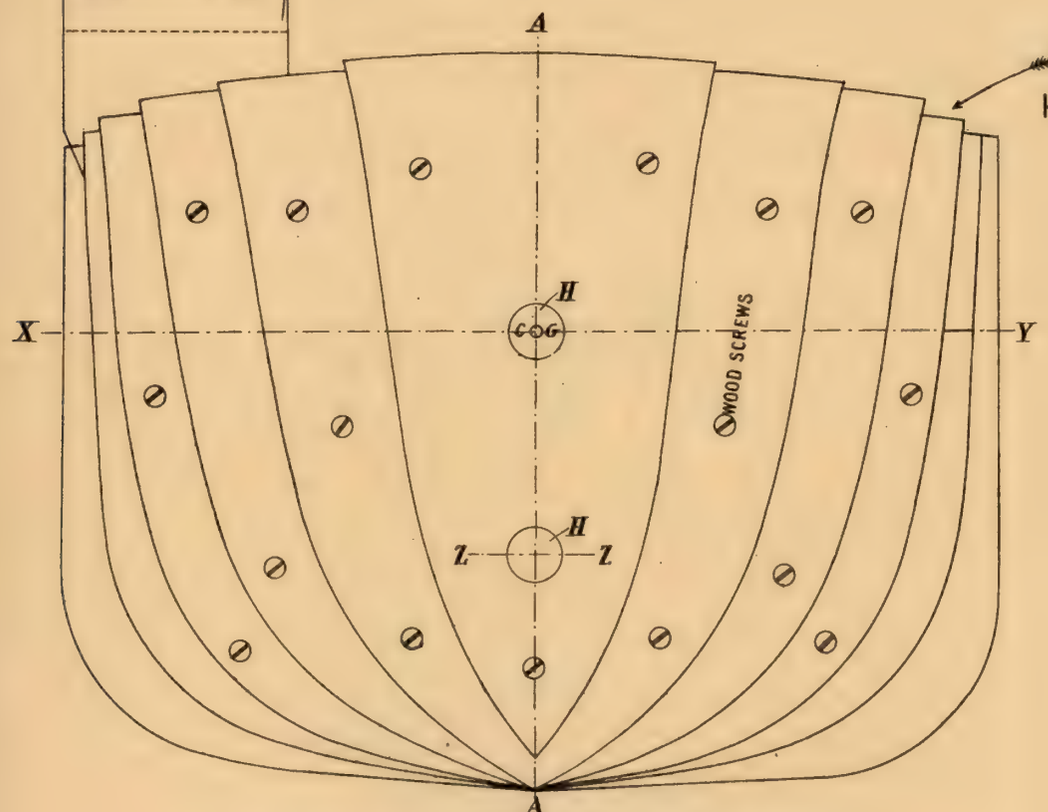

HOLES FOR ¼" BOLTS
SECTIONS ½ 1 2 3 4 5 6 7 8 9 10 10½
THICKNESS OF SECTIONS. ⅜ 9/16 ¾ ¾ ¾ ¾ ¾ ¾ ¾ ¾ ¾ 9/16 ⅜
INTERVAL OF SECTIONS. 6' 18' 24' 24' 24' 24' 24' 24' 24' 24' 24' 12' 6'

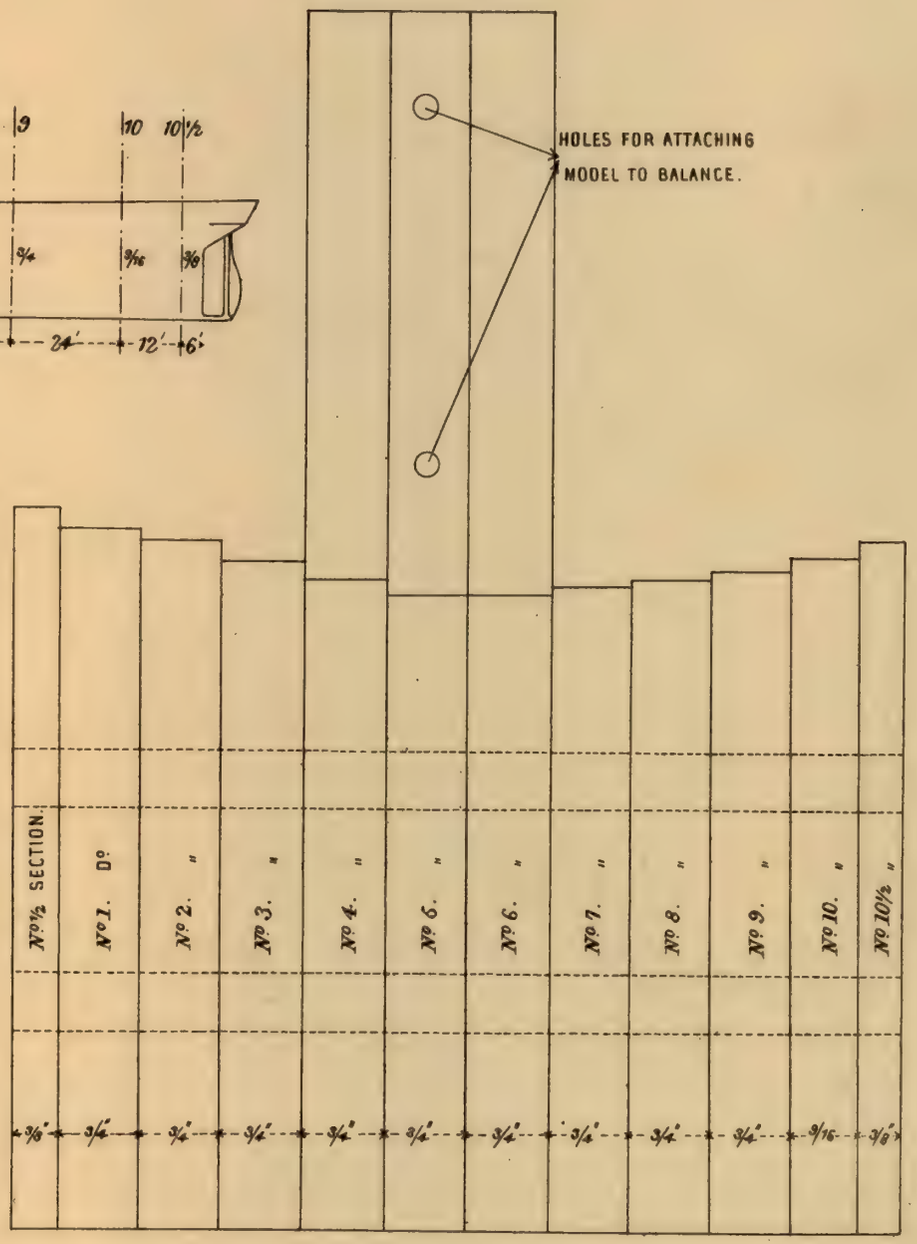
FIG. 3.



THE CENTRE OF GRAVITY SHOULD BE MARKED ON MODEL, IN ORDER THAT ITS DISTANCE FROM FULCRUM CAN BE MEASURED.

FIG. 2.

HALF FULL SIZE.



VESSEL 258 × 34 × 23.
MODEL MADE TO ¼ INCH ONE FOOT.

FIG. 4.

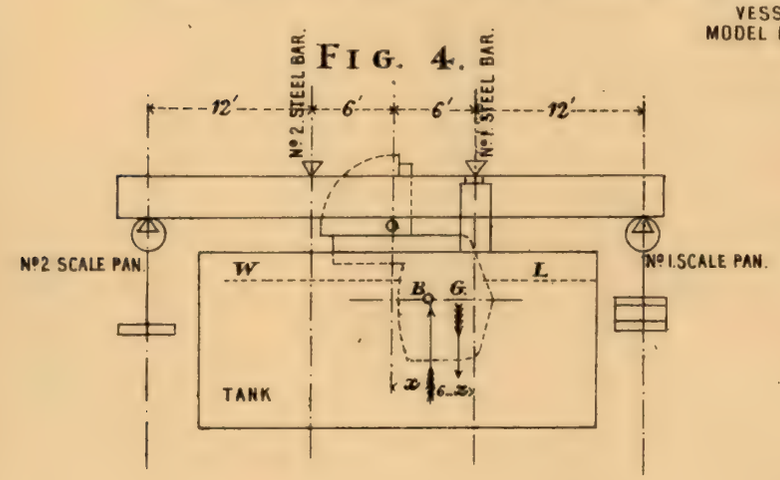


FIG. 5.

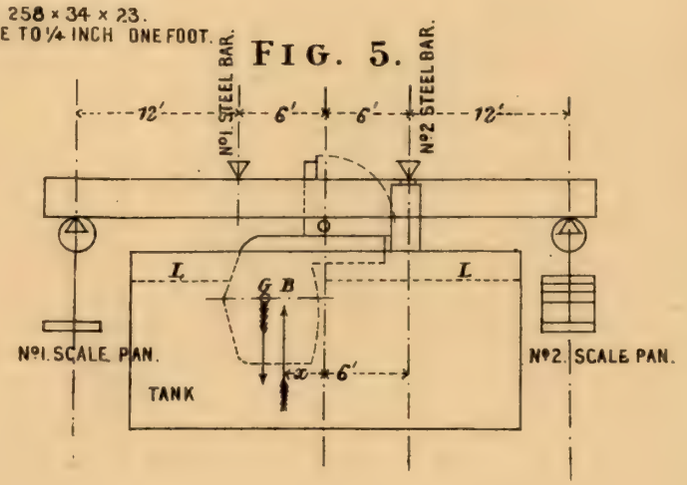
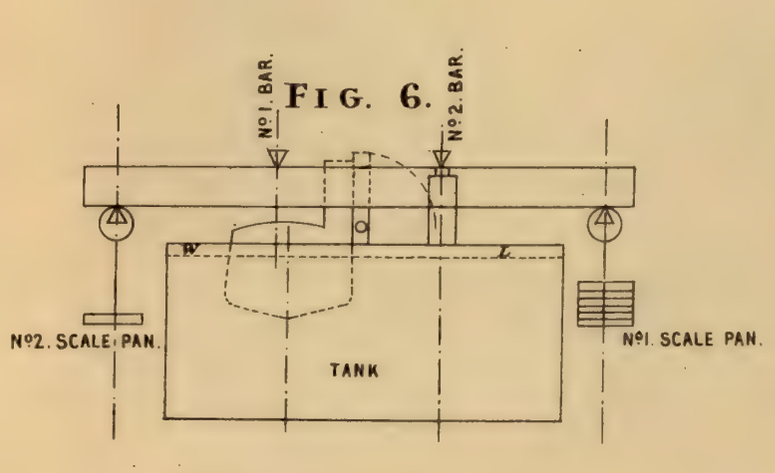
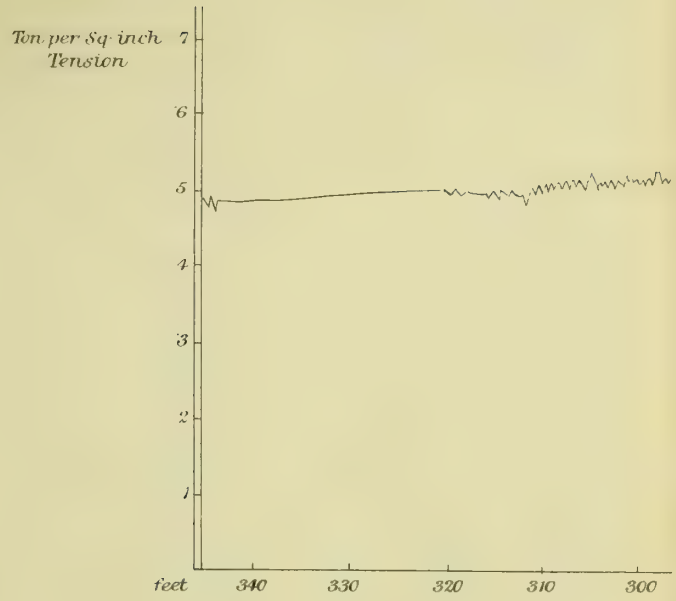
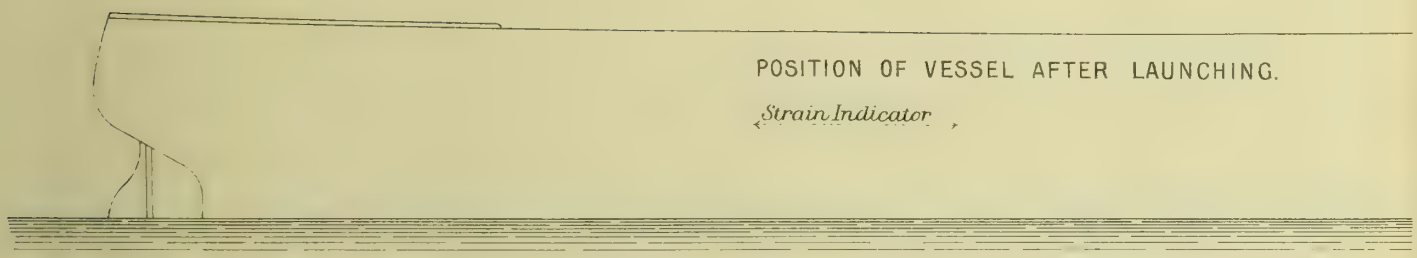


FIG. 6.







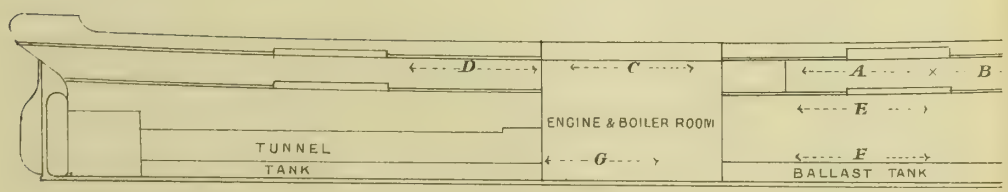
POSITION OF VESSEL AFTER LAUNCHING.

Strain Indicator

S. S. "COMPTON"

LENGTH 278½, BREADTH 34½, DEPTH 26¼.

Fig. 7a



LAUNCHING STRAINS.

Scale 1" = 20 ft.

Fig. 5.

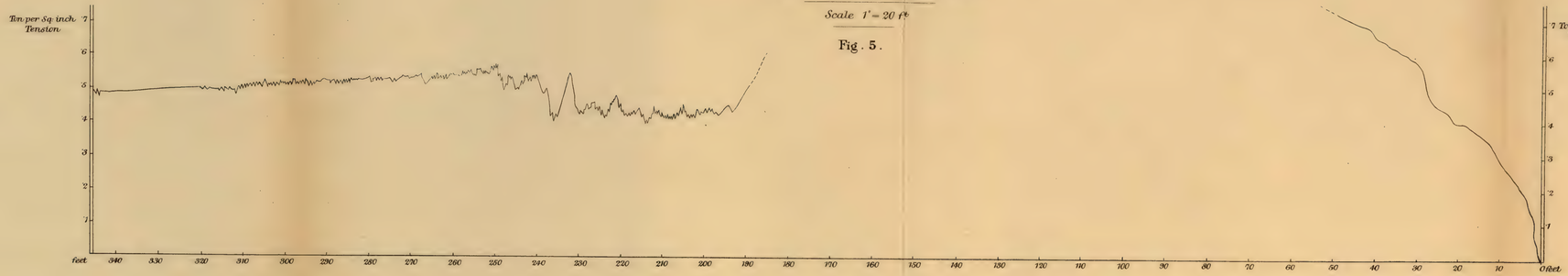
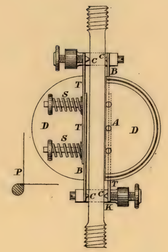
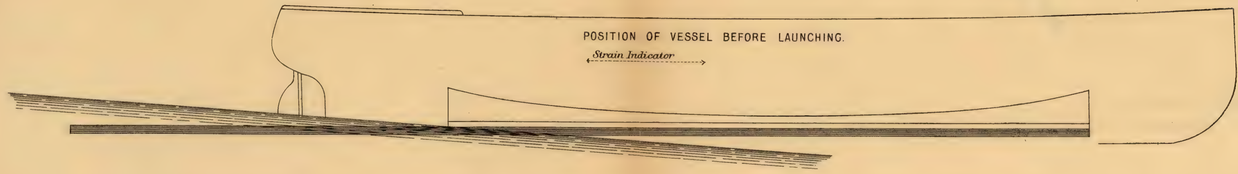


Fig. 3.

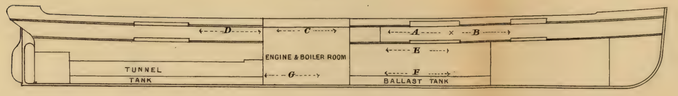




S. S. "COMPTON"

LENGTH 278ft, BREADTH 34ft, DEPTH 26ft.

Fig. 7a



S. S. "ALBATROSS."

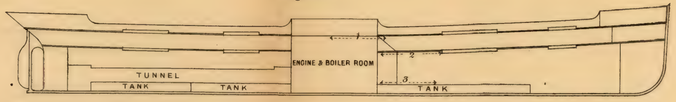
LENGTH 248·6, BREADTH 37, DEPTH 19·6.

Fig. 18a

S. S. "IDAS."

LENGTH 280, BREADTH 37, DEPTH 24·3

Fig. 19a



To Illustrate Mr. C. E. Stromeyer's Paper on a Strain Indicator for use at Sea.

Fig. 6.

Scale ⅛" = 1 ft.

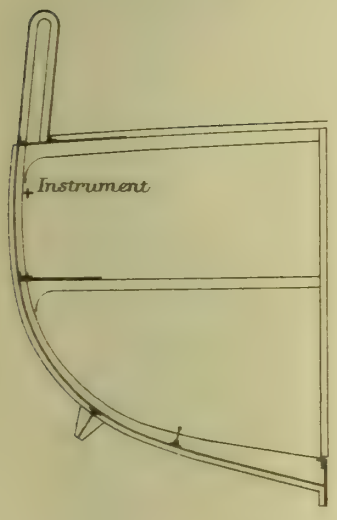


Fig. 7.

S.S "COMPTON"

LENGTH 278½, BREADTH 34½, DEPTH 26¼.

Scale ⅛" = 1 ft.

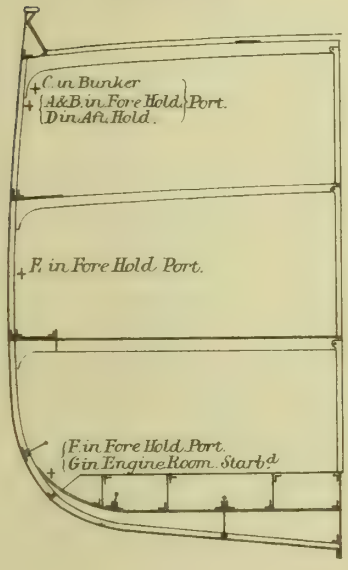


Fig. 8.

S.S. "SILVERTOWN" LATE "HOOPER."

LENGTH 338·2, BREADTH 55, DEPTH 34·6.

Scale ⅛" = 1 ft.

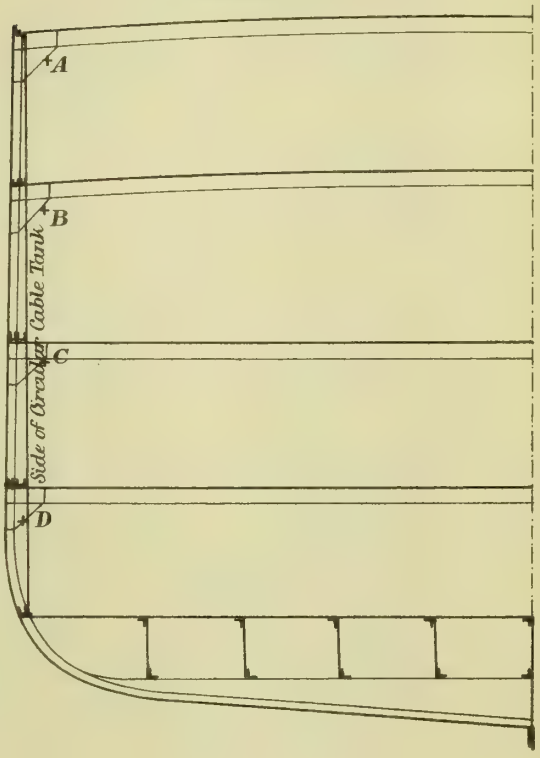


Fig. 17.

S.S. "IDAS."

LENGTH 280, BREADTH 37, DEPTH 24·3.

Scale ⅛" = 1 ft.

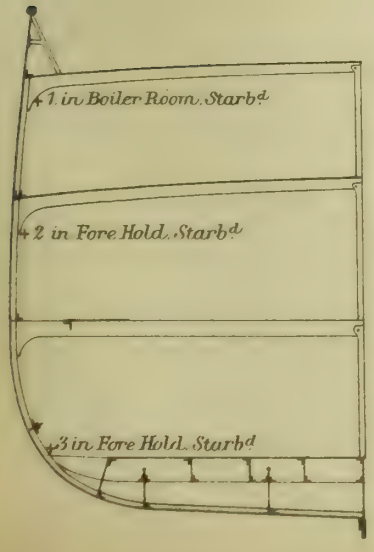


Fig. 18.

S.S. "ALBATROSS"

LENGTH 248·6, BREADTH 37, DEPTH 19·7.

Scale ⅛" = 1 ft.

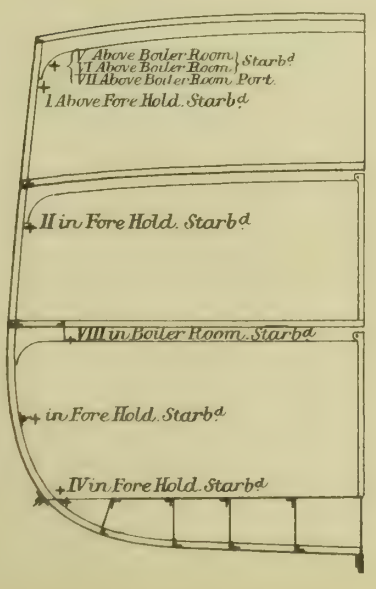
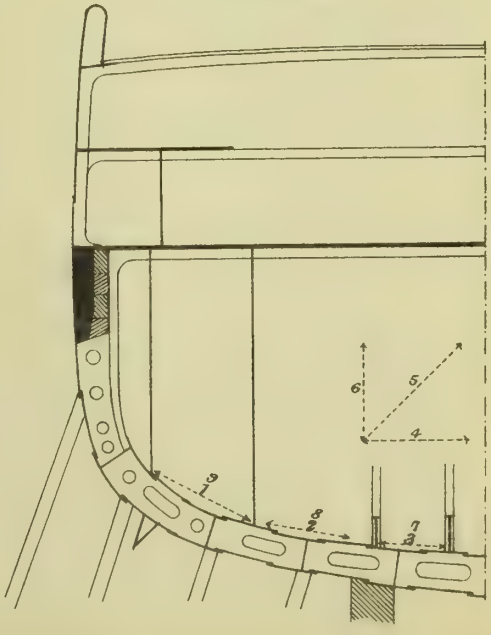

V Above Boiler Room } Starbd
I Above Boiler Room }
VII Above Boiler Room Port
I Above Fore Hold Starbd
II in Fore Hold Starbd
VIII in Boiler Room Starbd
in Fore Hold Starbd
IV in Fore Hold Starbd

Fig. 19.

H.M.S. "BENBOW."

POSITION OF STRAIN INDICATORS.

Scale 1/16" = 1 ft.



To Illustrate Mr. C. E. Stromeyer's Paper on a Strain Indicator for use at Sea.

S. S. "HOOPER"
DOCKING STRAINS.

SCALE OF READINGS.
10 = 90° ON DIAL.
+ 1 ON DIAL = .0027" PER 24 FT SPAN = 1 PER 77,500 ELONGATION

FIG. 9.

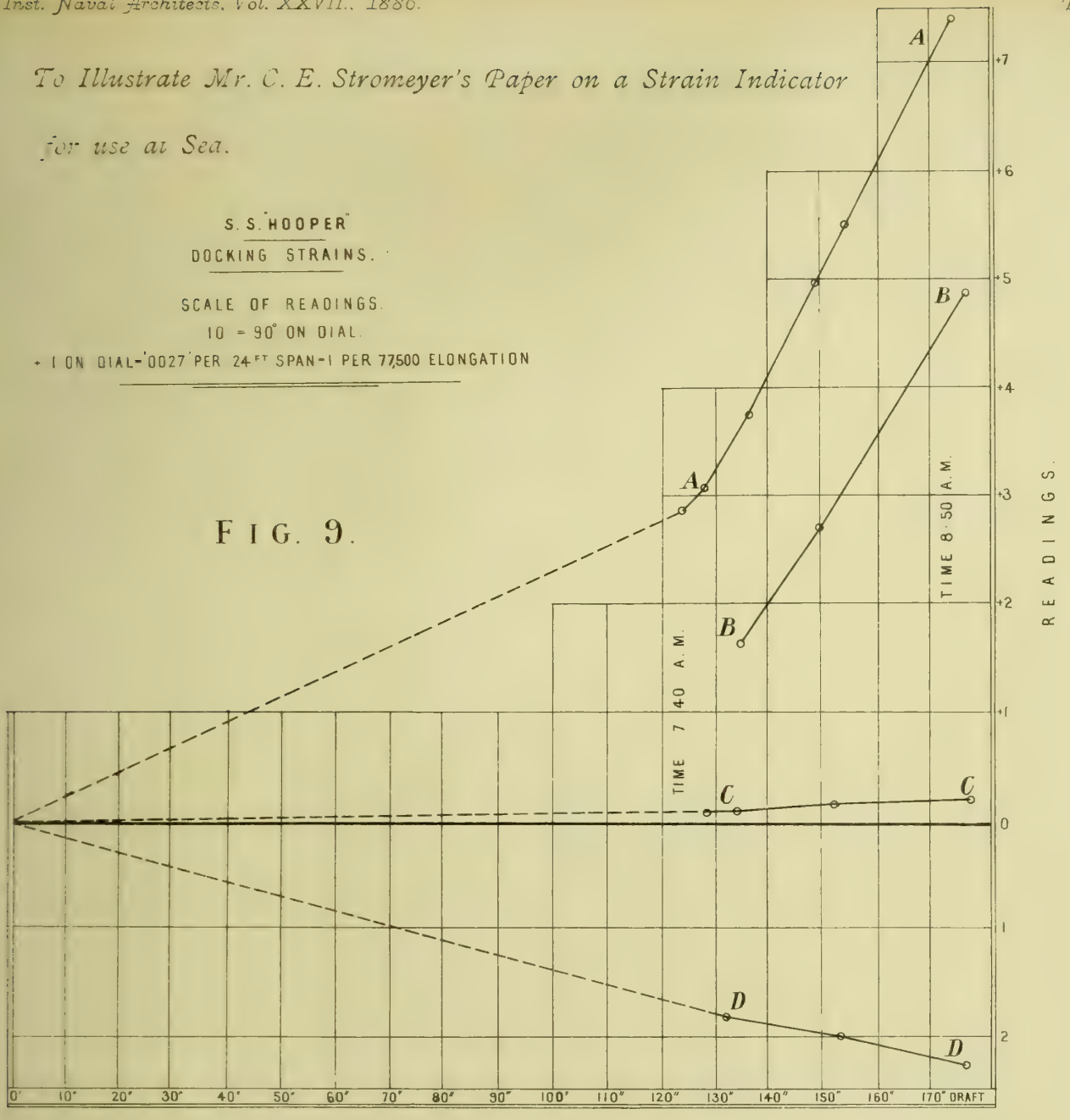
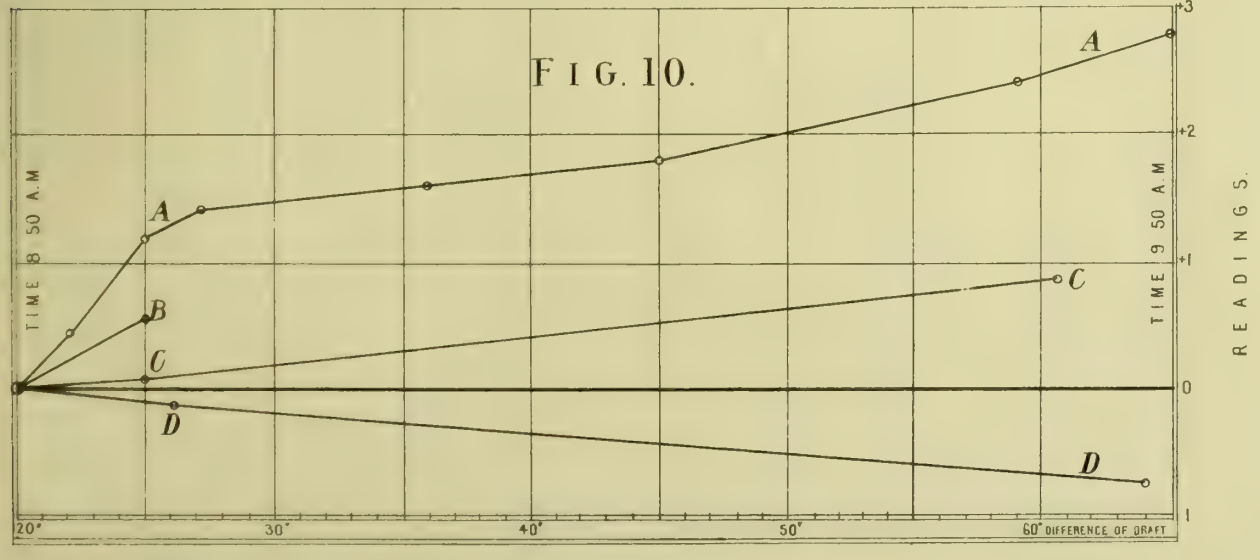


FIG. 10.



To Illustrate Mr. C. E. Stromeyer's Paper on a Strain Indicator for use at Sea.

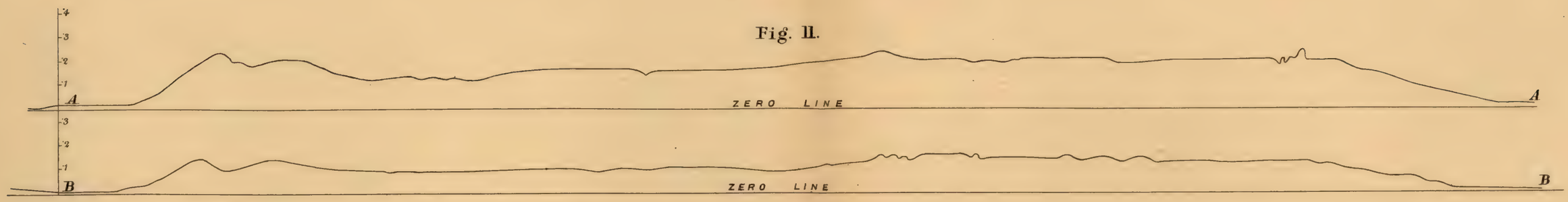


Fig. 11.

Goods Train from Hamburg. Time 90 Sec. Weight 511½ Tons. Length 790 ft.

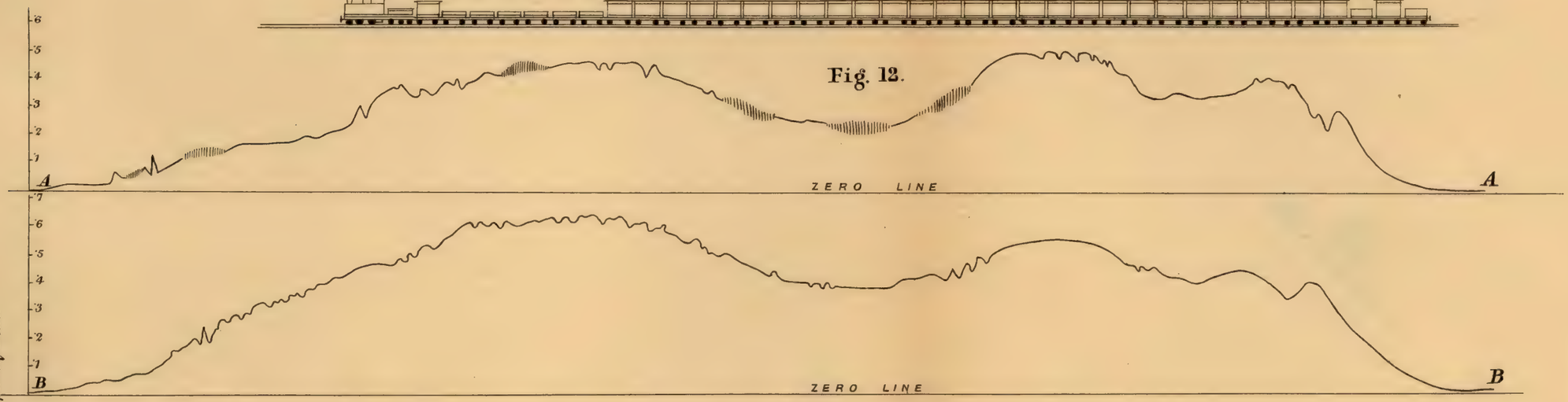
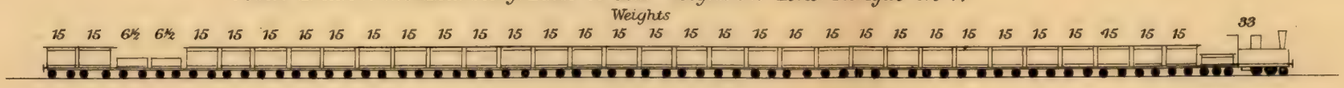


Fig. 12.

Goods Train from Haarburg. Time 70 Sec. Weight 511 Tons. Length 710 ft.



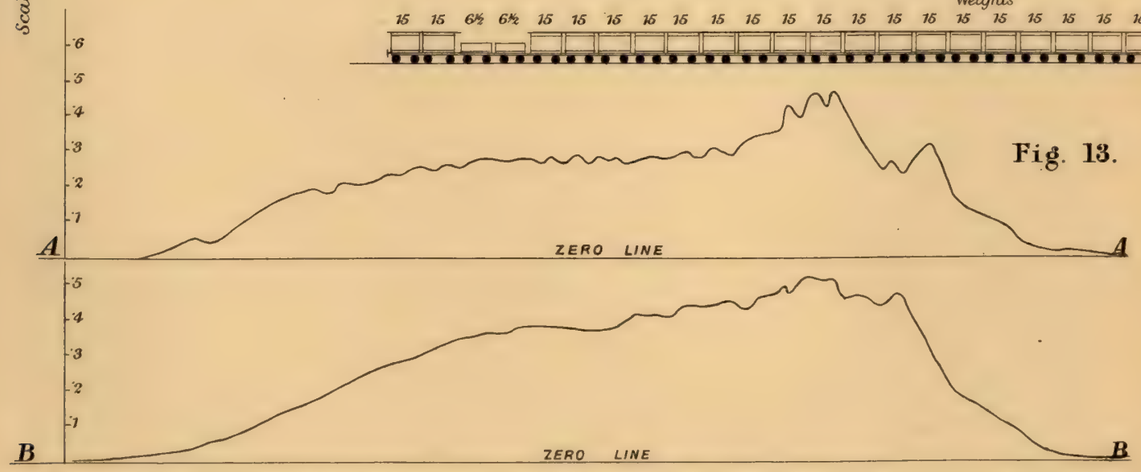
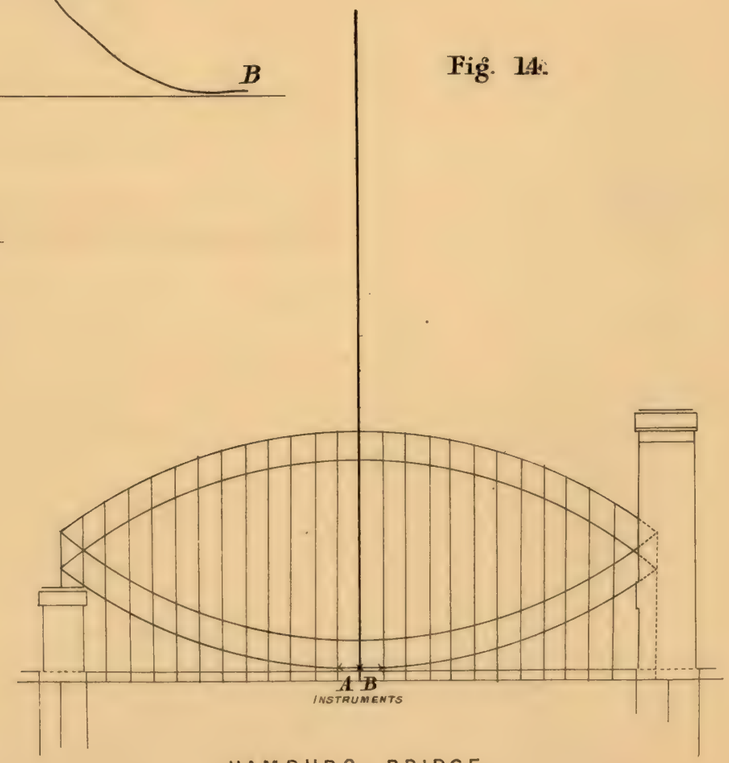


Fig. 13.

Total Time 15 Sec. Passenger Train from Haarburg. Weight 104¾ Tons. Length 260 ft.

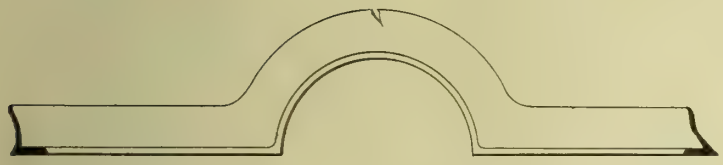


HAMBURG BRIDGE.

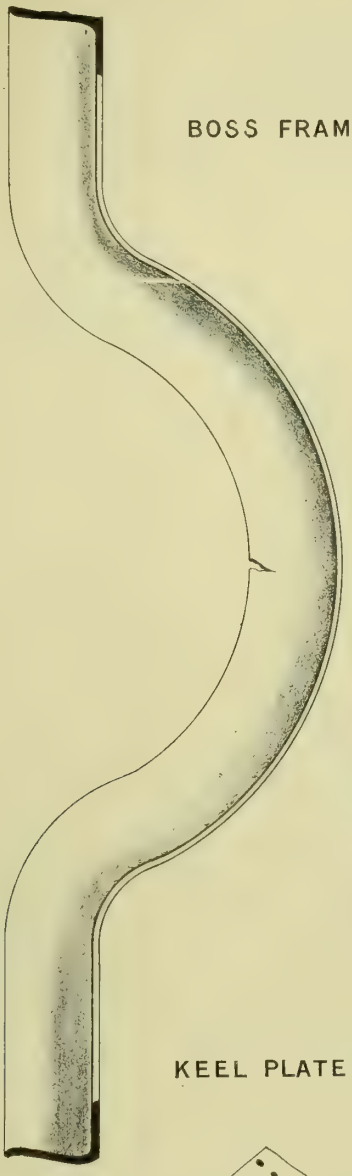
Fig. 14.

To Illustrate Mr. J. Ward's Paper on the present Aspect of Mild Steel for Shipbuilding.

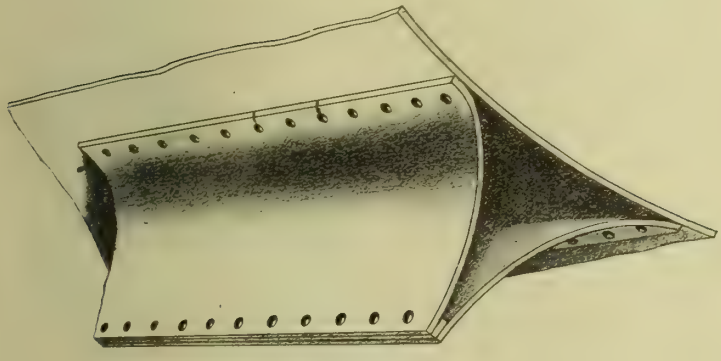
HEEL PIECE



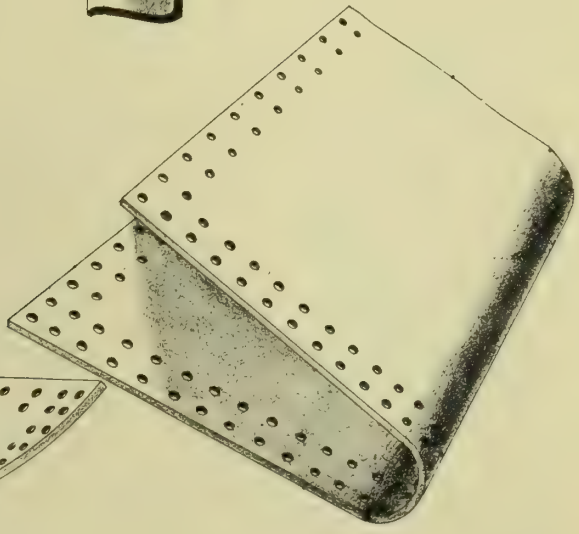
BOSS FRAME



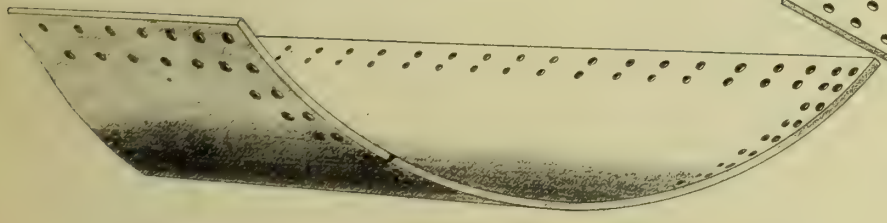
BILGE KEEL



KEEL PLATE AFT



KEEL PLATE



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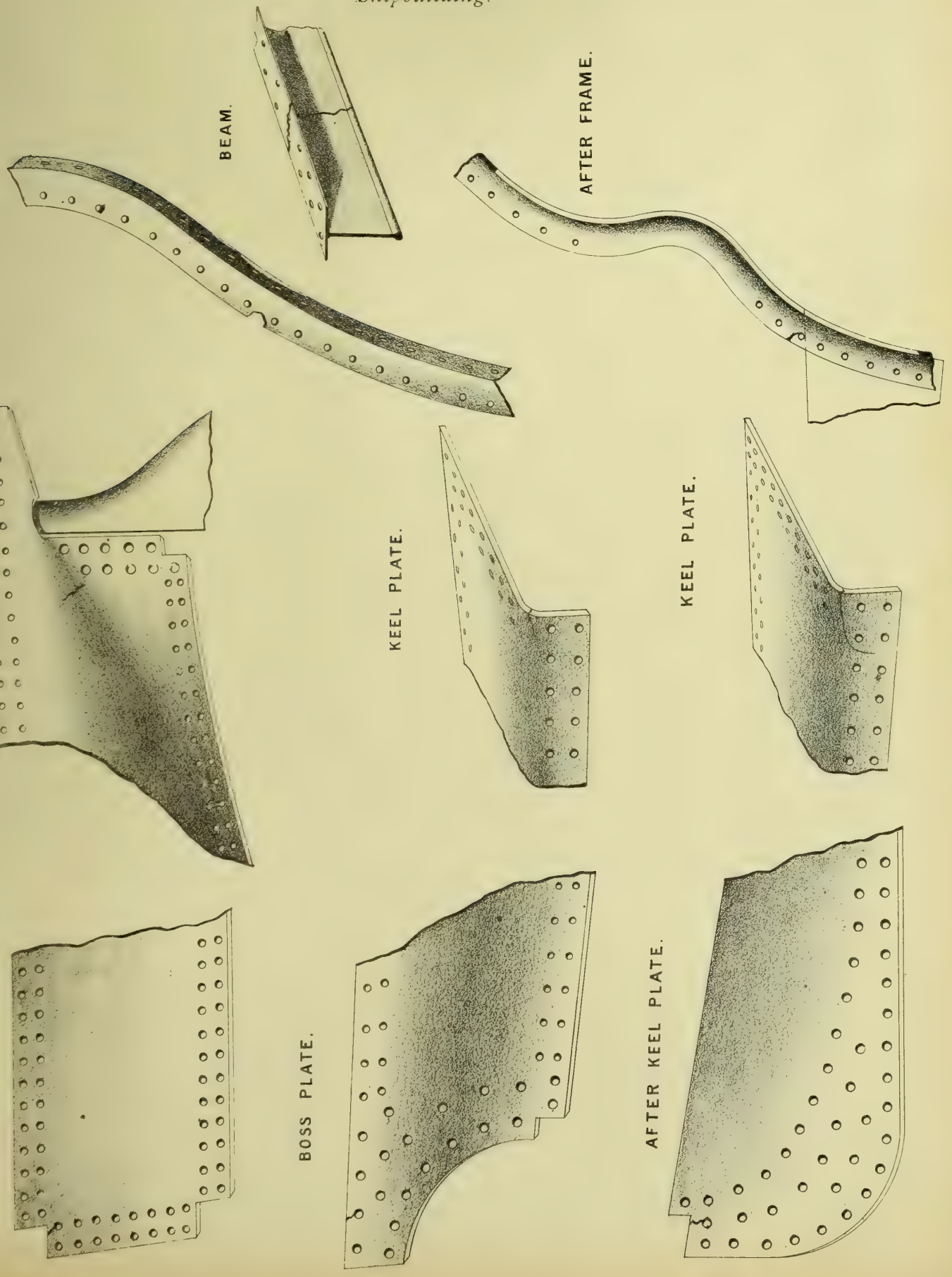

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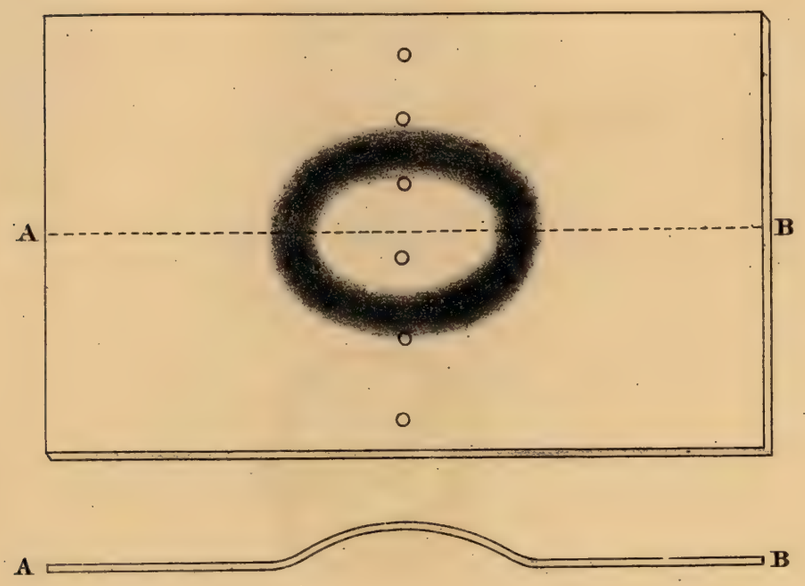
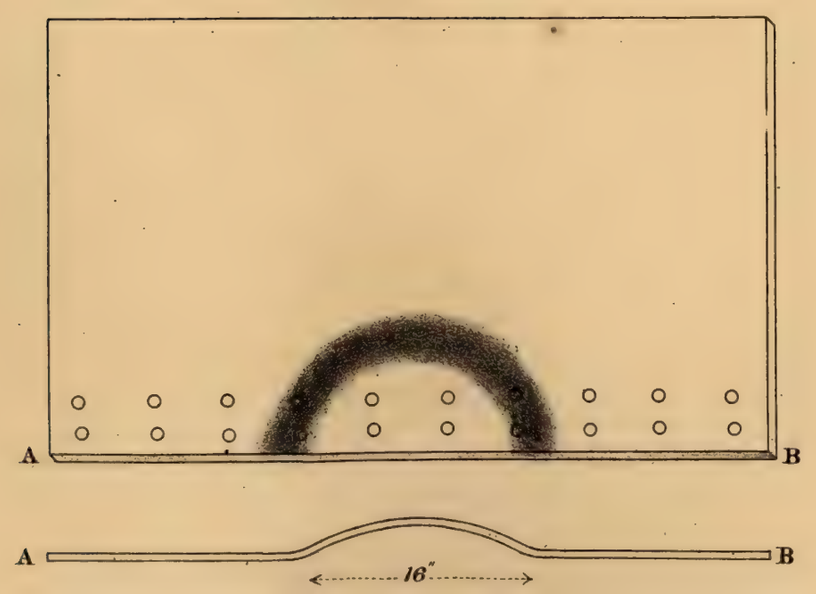
LOCAL HEATING Nº 1.

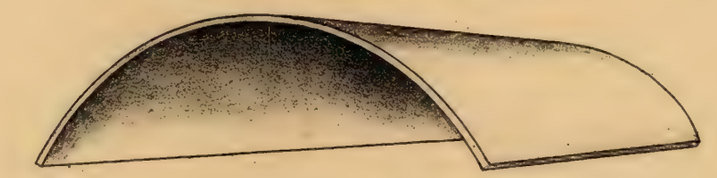


LOCAL HEATING Nº 2.



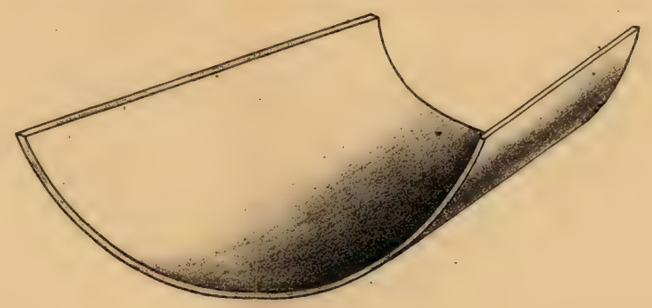
LOCAL HEATING Nº 3.
PLATE BEFORE TREATMENT
AND
AFTER TREATMENT.

ROLLED COLD.



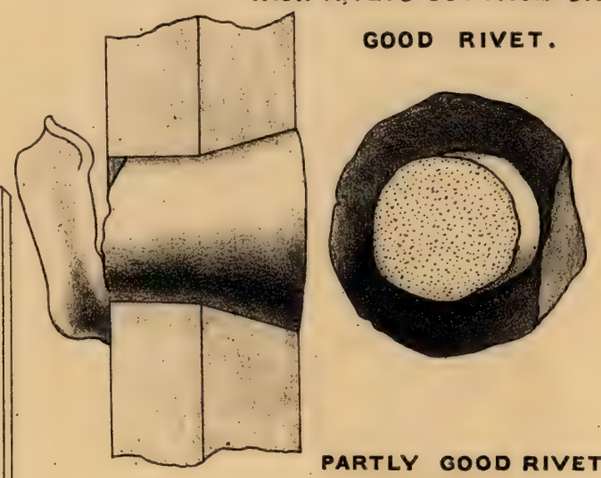
LOCALLY HEATED & DISHED

LOCALLY HEATED & BENT TO REVERSE CURVE.



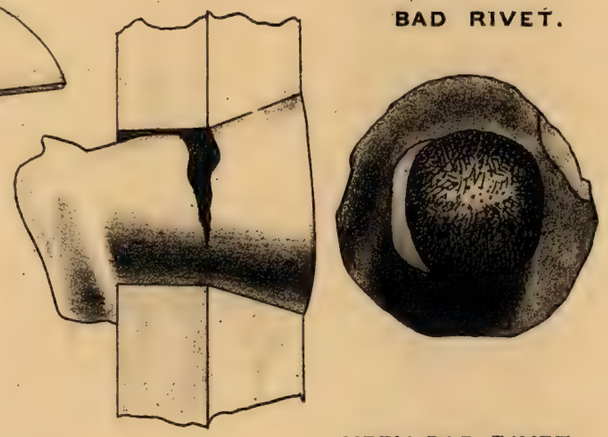
IRON RIVETS CUT FROM STEEL PLATES.

GOOD RIVET.

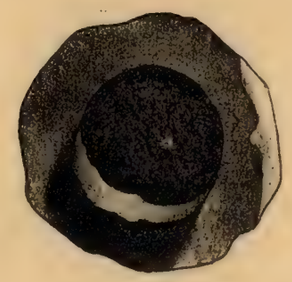


PARTLY GOOD RIVET.

BAD RIVET.



VERY BAD RIVET.



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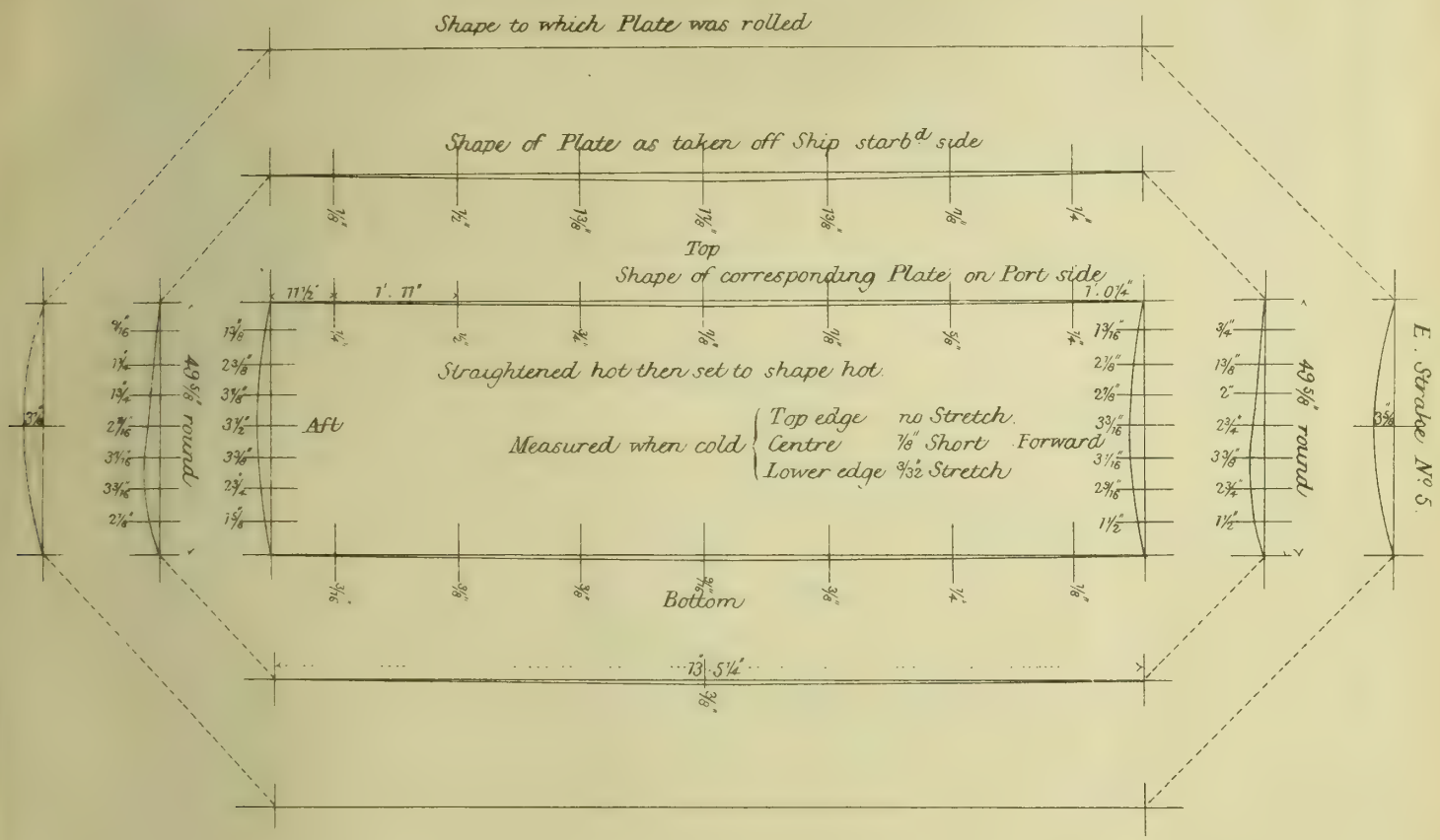

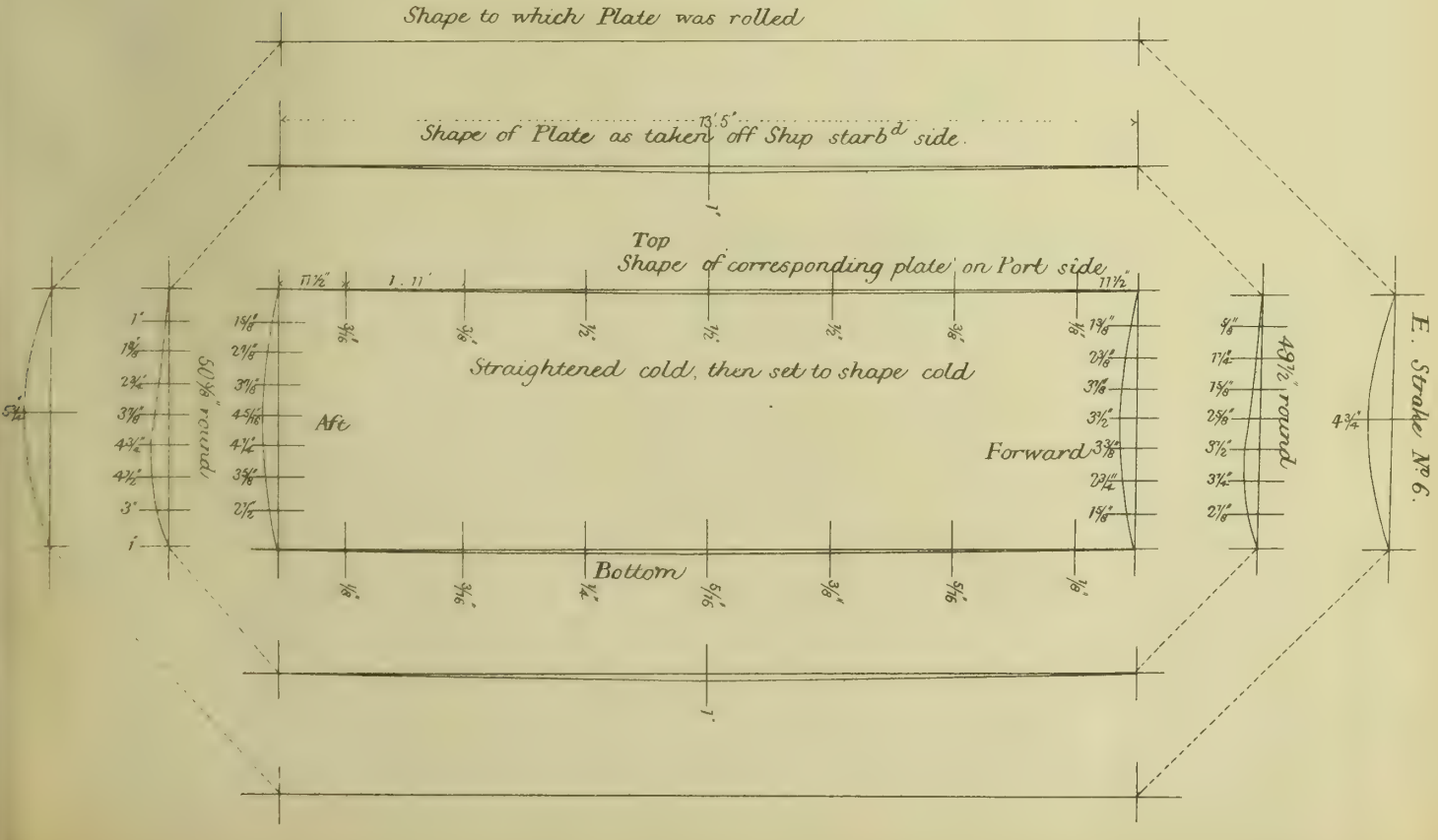

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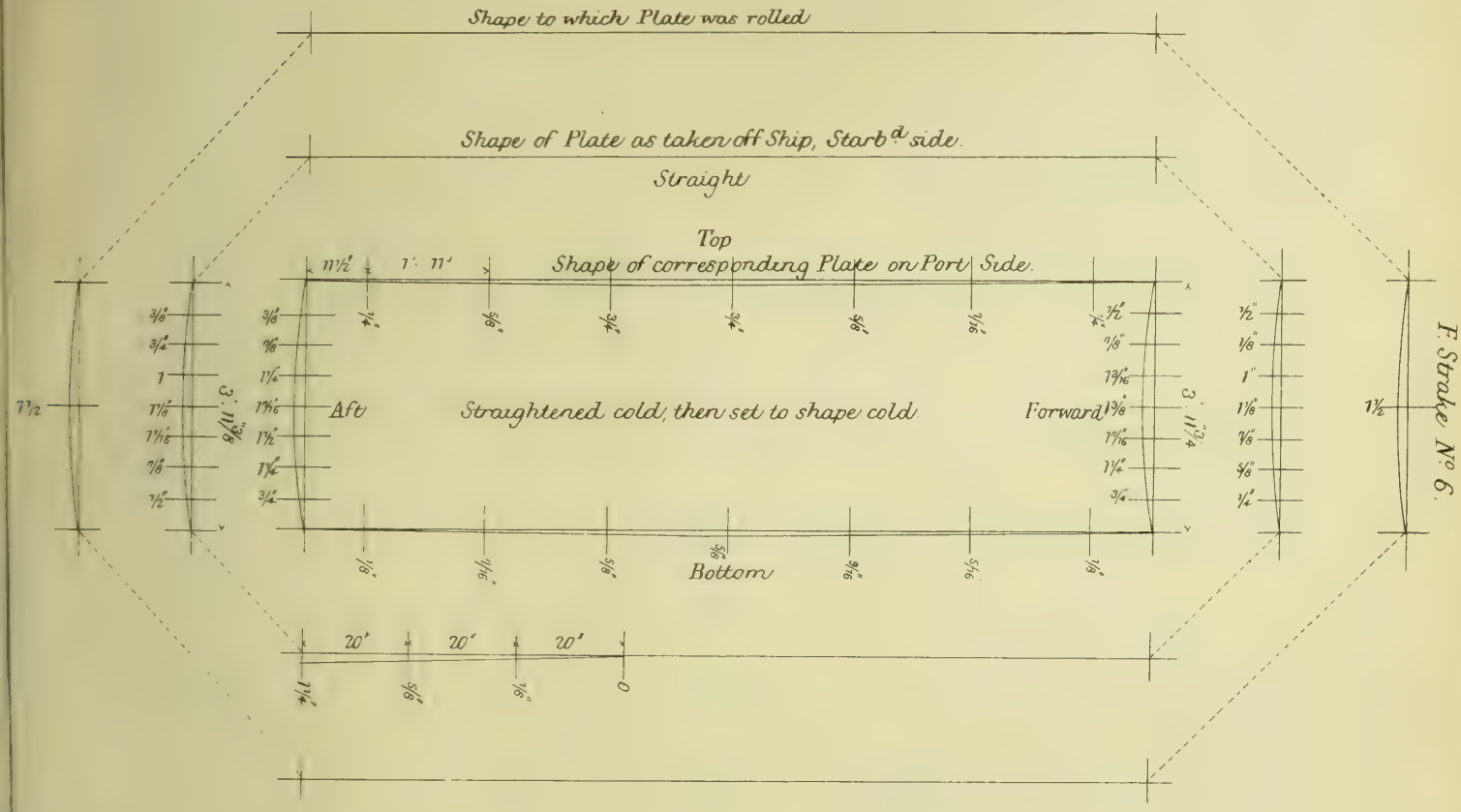

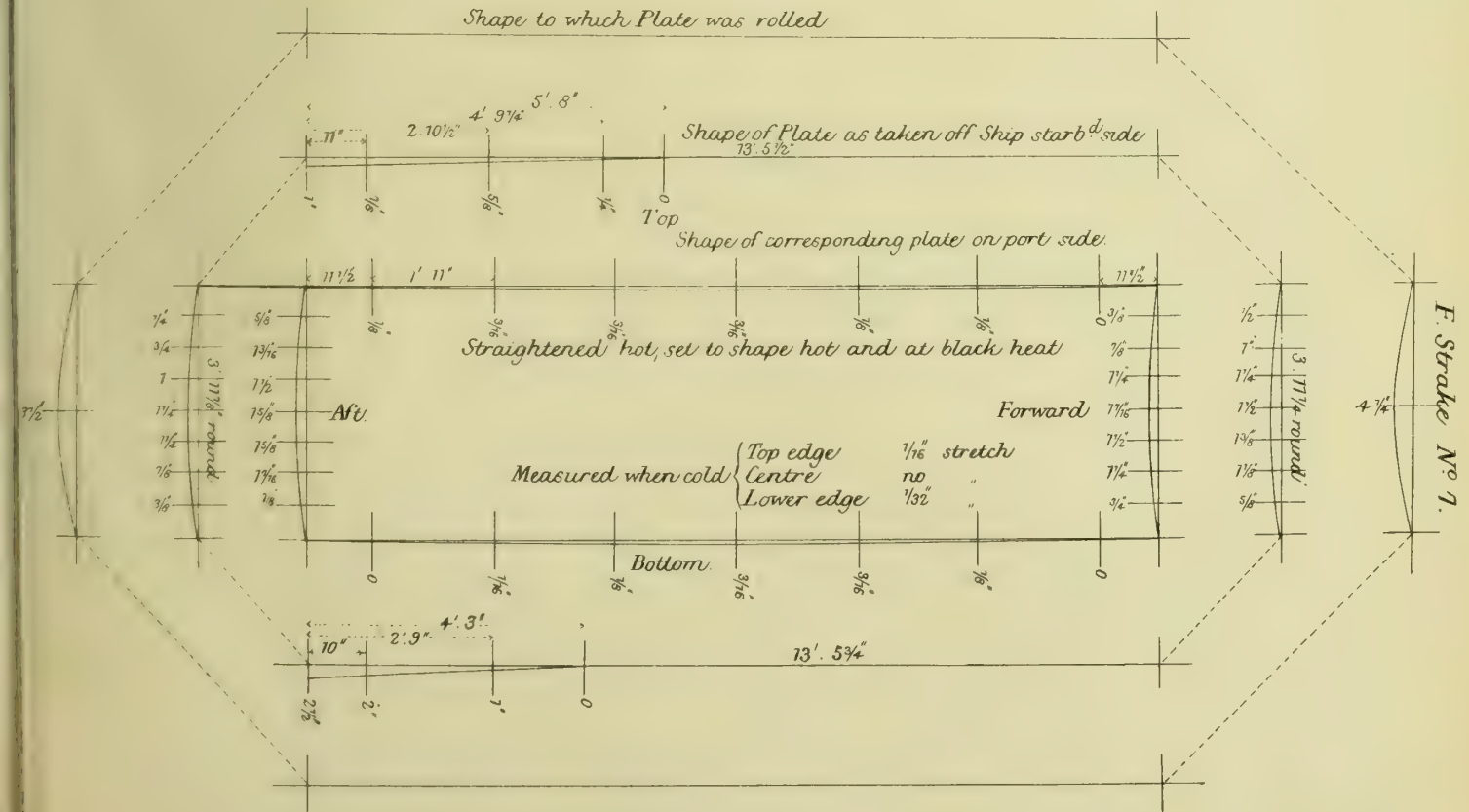
Shape to which Plate was rolled
Shape of Plate as taken off Ship starb^d side
Top
Shape of corresponding plate on port side
Straightened hot, set to shape hot and at black heat
Aft
Forward
Measured when cold
Top edge 1/16" stretch
Centre no "
Lower edge 1/32" "
Bottom
F. Strake No 7.

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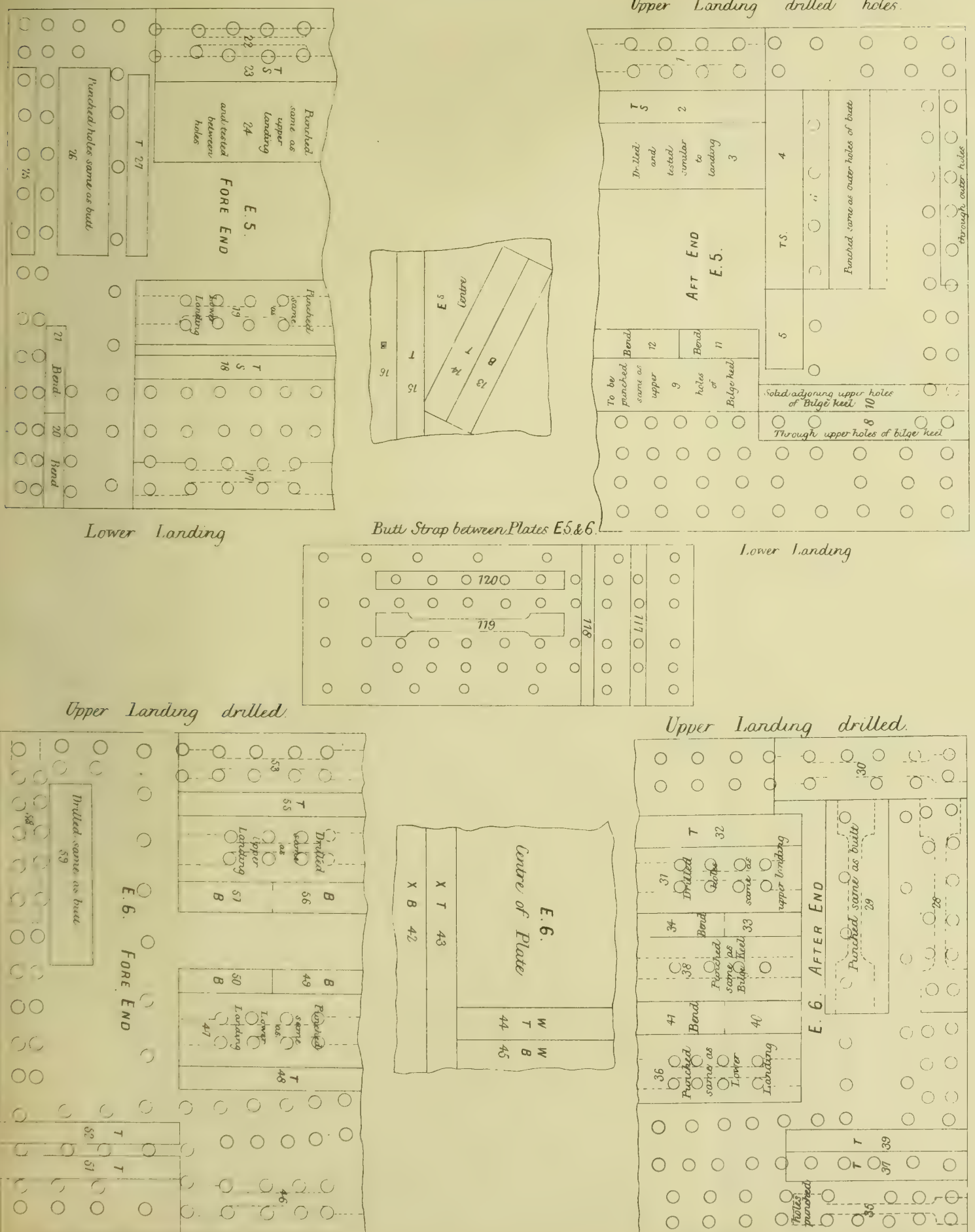
Upper Landing
Upper Landing drilled holes
Fore Butt
After Butt
E.5. Fore End
E.5. Aft End
Punched same as upper landing
Punched holes same as butt
Drilled and tested similar to landing
Punched same as outer holes of butt
Through outer holes
To be punched same as upper holes of Bilge keel
Solid adjoining upper holes of Bilge keel
Through upper holes of bilge keel
Lower Landing
E.5. Centre
Butt Strap between Plates E5 & E6
Upper Landing drilled
Upper Landing drilled
E.6. Fore End
E.6. Centre of Plate
E.6. After End
Drilled same as butt
Punched same as built
Punched same as Bilge keel
Punched same as Lower Landing
Lower Landing punched
Lower Landing punched
After Butt punched

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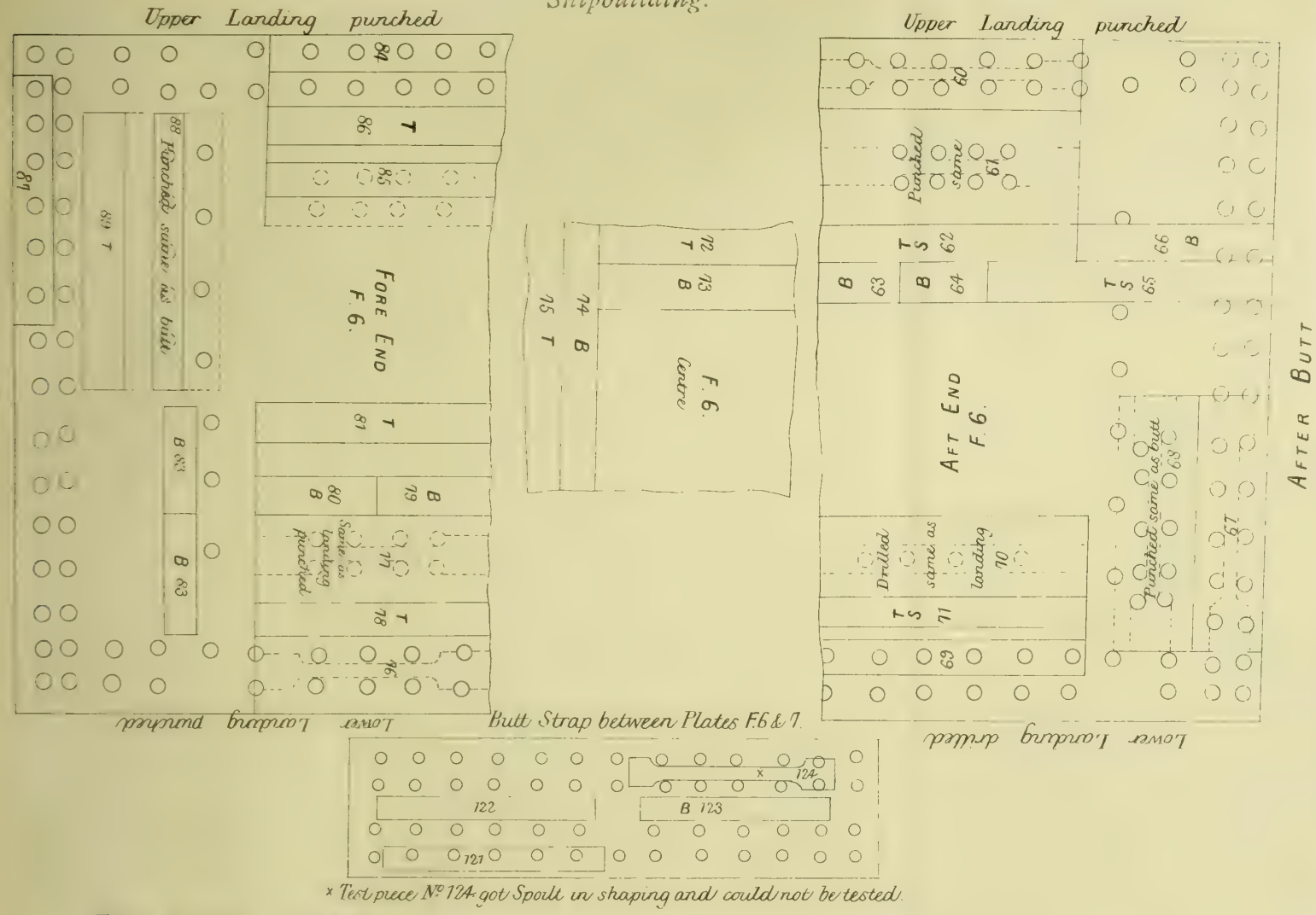
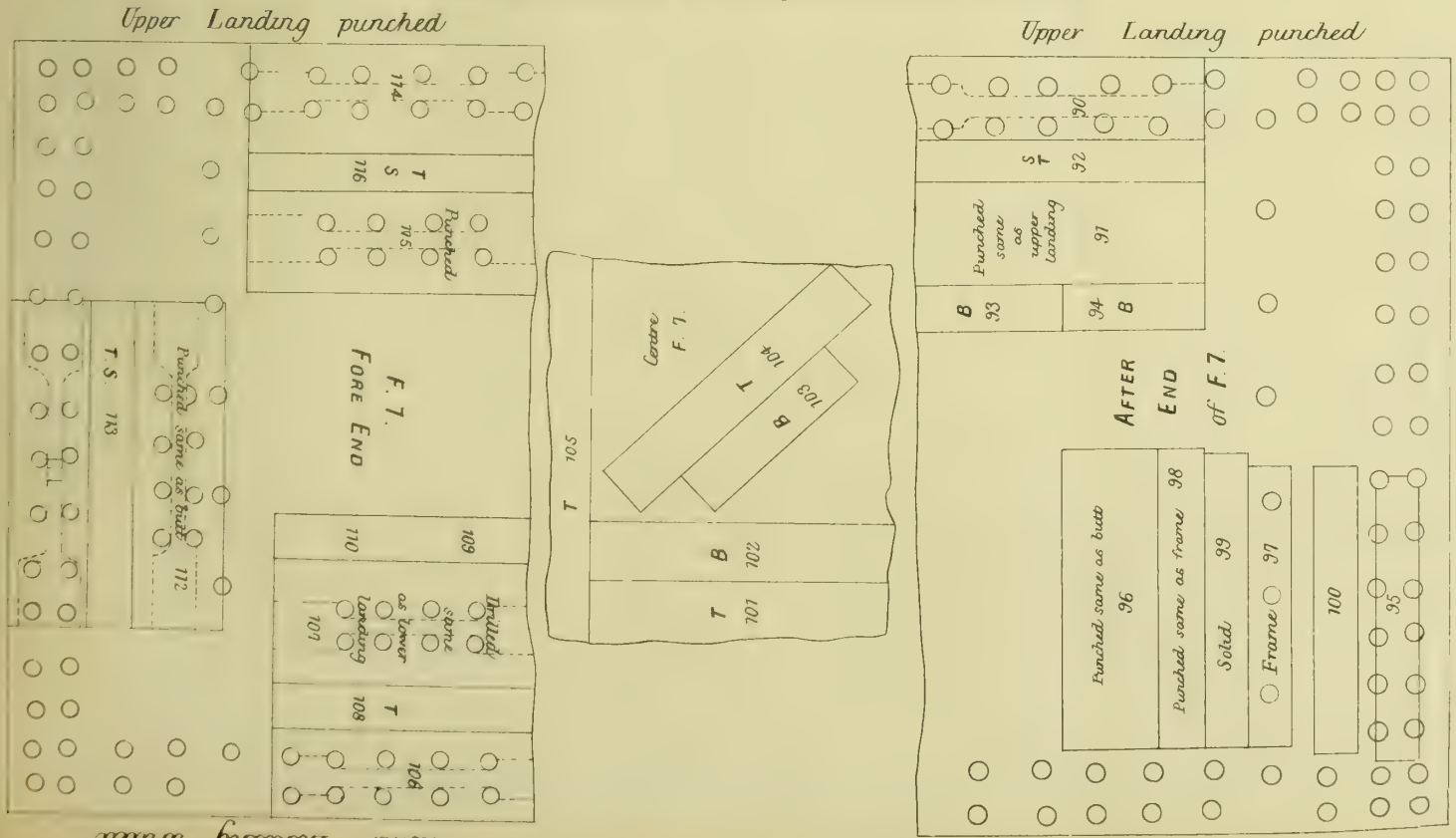


Butt Strap between Plates F.6 & 7.

× Test piece No 124 got Spoilt in shaping and could not be tested.



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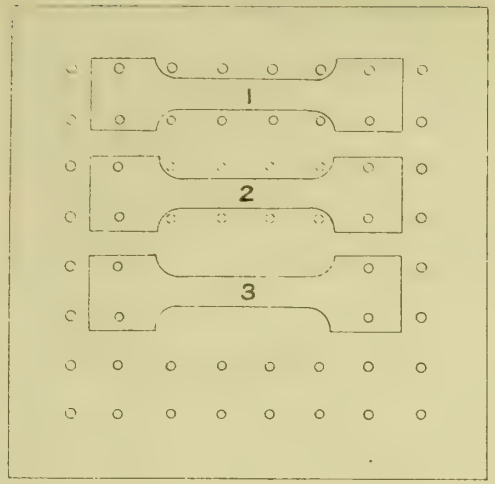
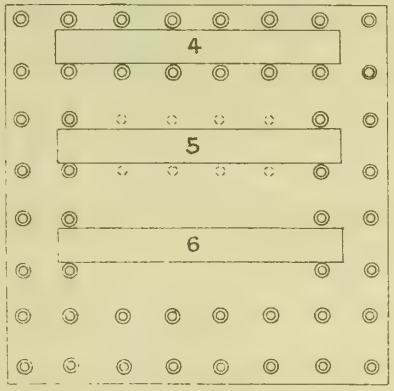
RIVETED ONCE.

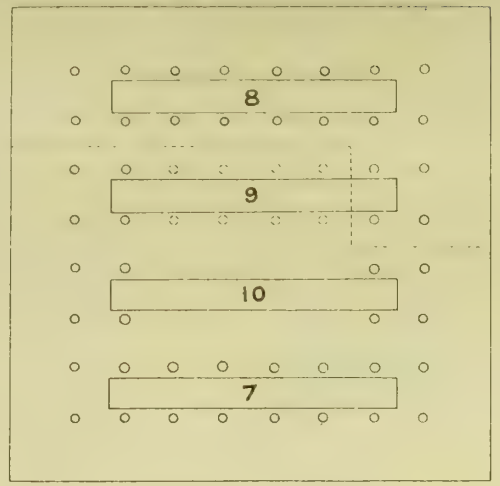


A.A.

RIVETED ONCE.



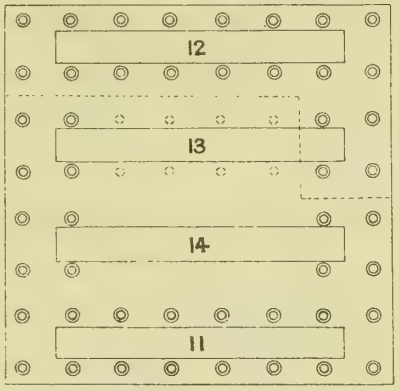
RIVETED 3 TIMES.



RIVETED TWICE.

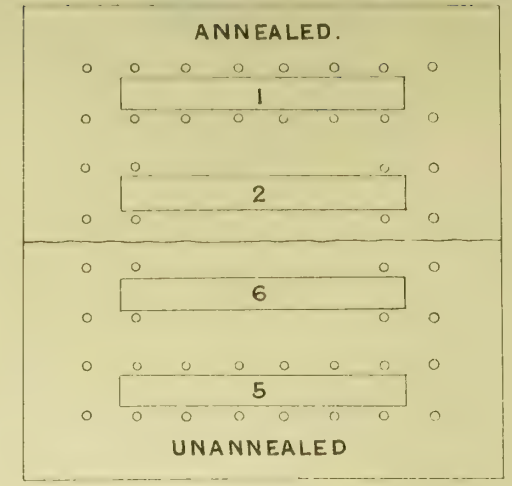
E.E.

RIVETED 3 TIMES.



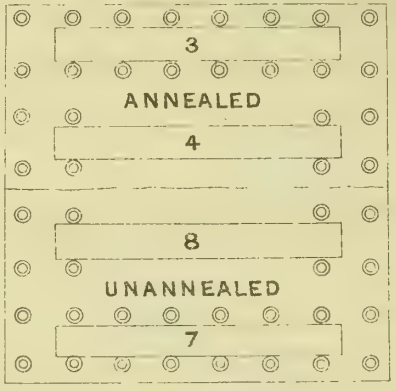
RIVETED TWICE.

RIVETED 3 TIMES.

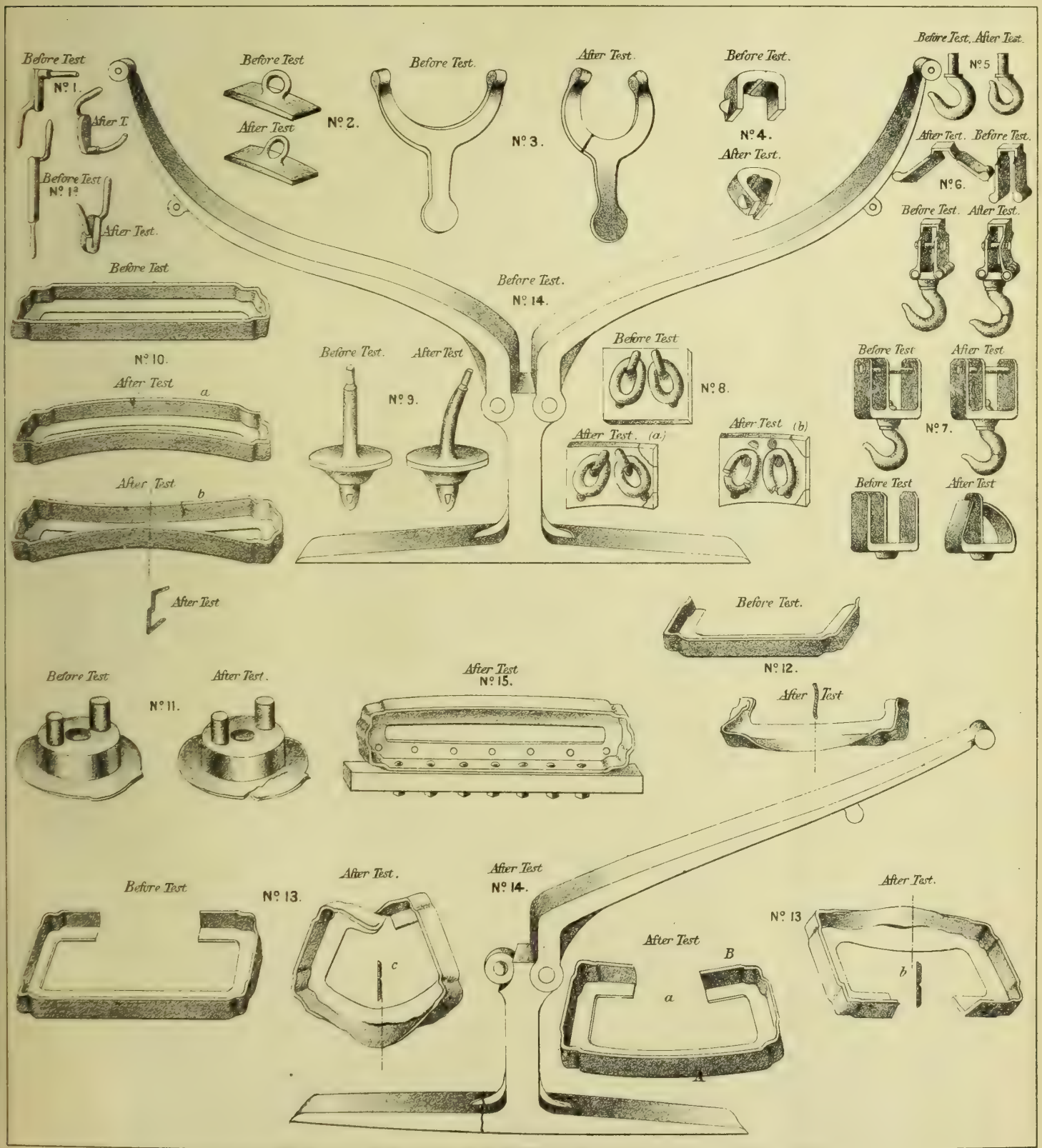


M

RIVETED 3 TIMES.



To Illustrate Mr. E. C. Warren's Paper on the Use of Steel Castings in Lieu of Iron Forgings and Brass Castings.



To Illustrate Mr. E. C. Warren's Paper on the Use of Steel Castings in Lieu of Iron Forgings and Brass Castings.

Fig. 2.
SKETCH OF CAST STEEL STEM FOR A FOREIGN VESSEL.
Scale ½ Inch = 1 foot

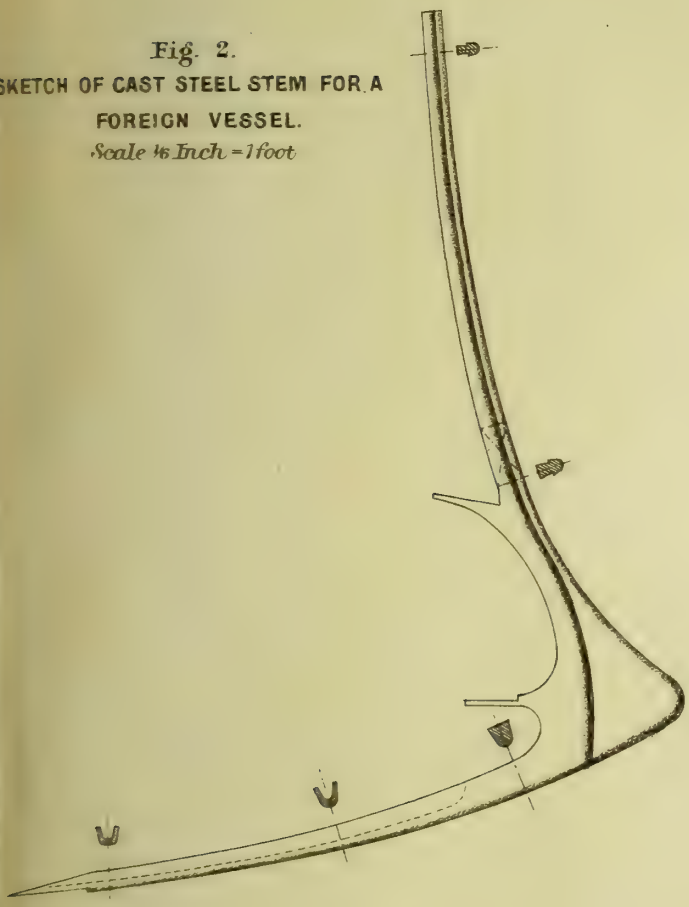


Fig. 3.
SKETCH OF CAST STEEL STEM FOR A TORPEDO VESSEL.
Scale ½ Inch = 1 foot.

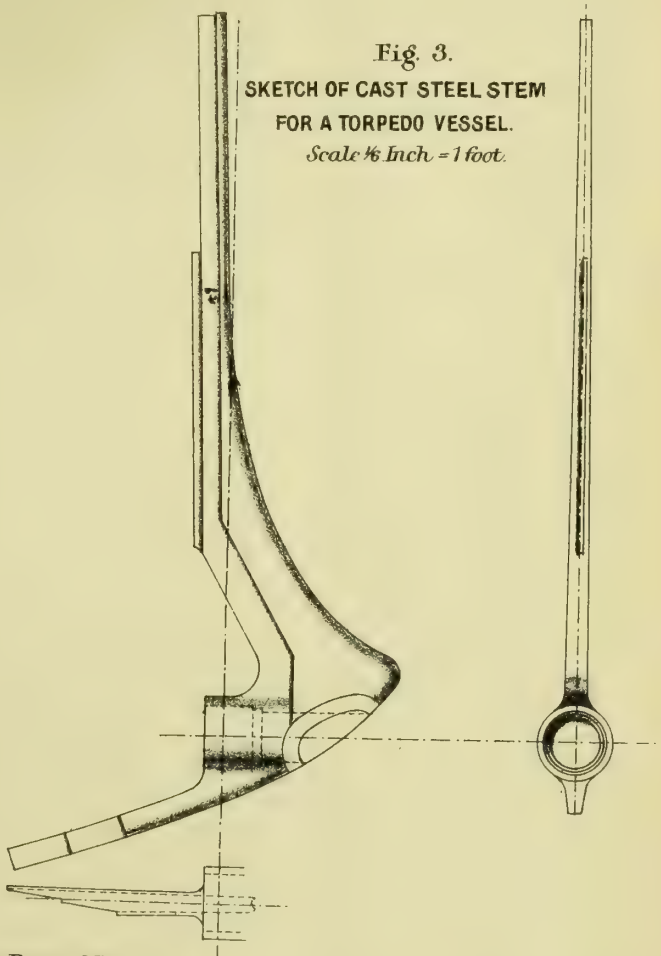


Fig. 4.
SKETCH OF CAST STEEL STERN POST FOR A FOREIGN VESSEL.
Scale ½ Inch = 1 foot.

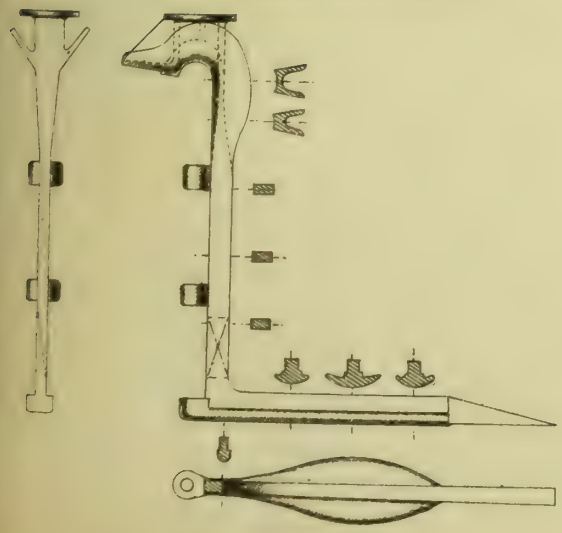
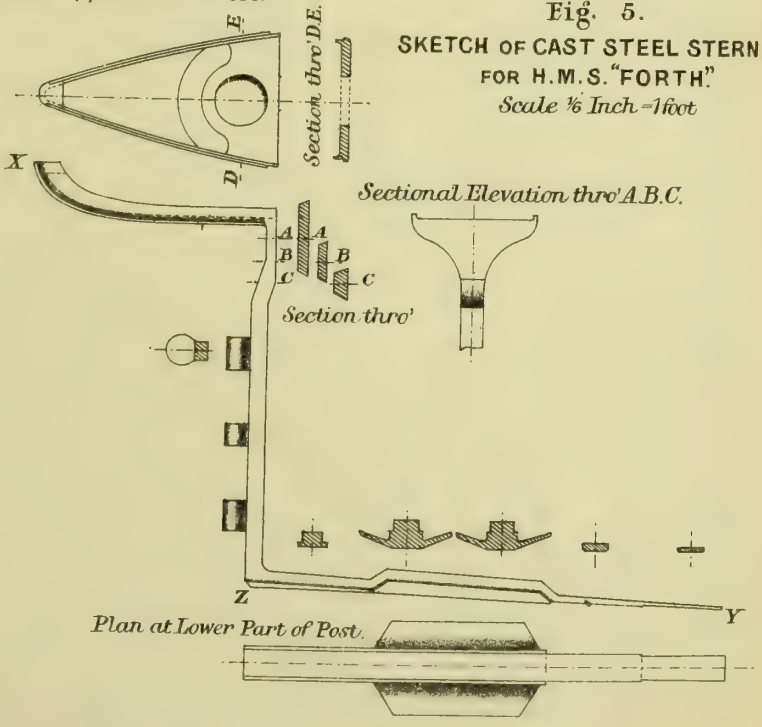
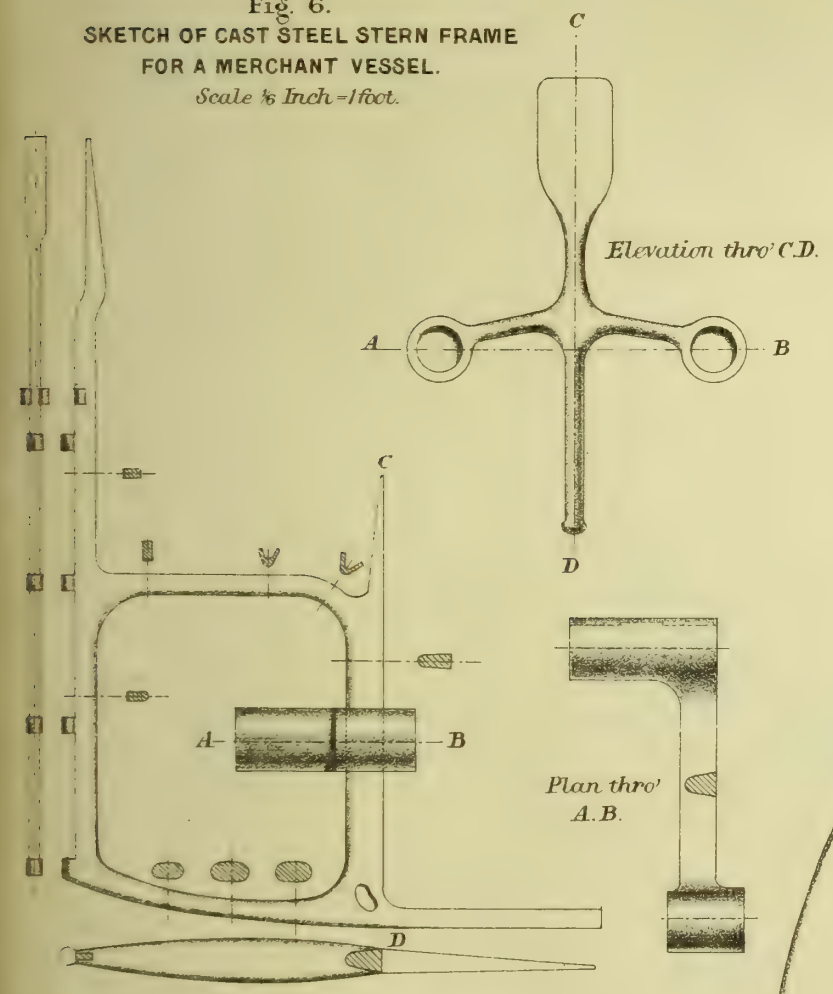


Fig. 5.
SKETCH OF CAST STEEL STERN POST FOR H.M.S. "FORTH."
Scale ½ Inch = 1 foot



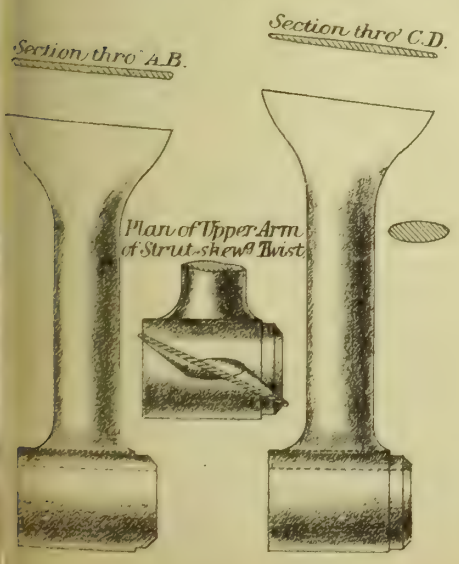
To Illustrate Mr. E. C. Warren's Paper on the Use of Steel Castings in Lieu of Iron Forgings and Brass Castings

Fig. 6.
SKETCH OF CAST STEEL STERN FRAME FOR A MERCHANT VESSEL.
Scale ⅛ Inch = 1 foot.



SKETCH OF CAST STEEL STERN BRACKETS FOR SUPPORTING THE AFTER END OF PROPELLOR SHAFT OF H.M.S. "BENBOW."
Scale ⅛ Inch = 1 foot.

Fig. 8.



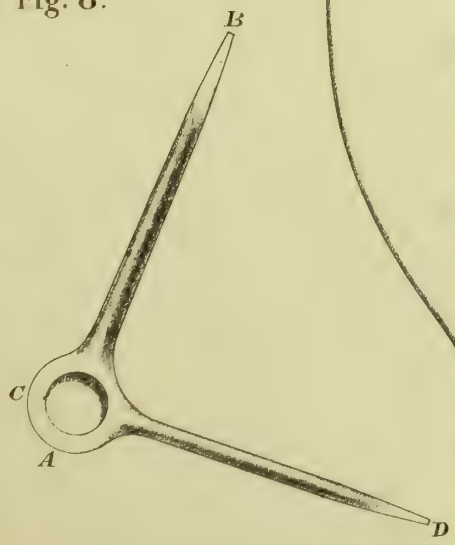
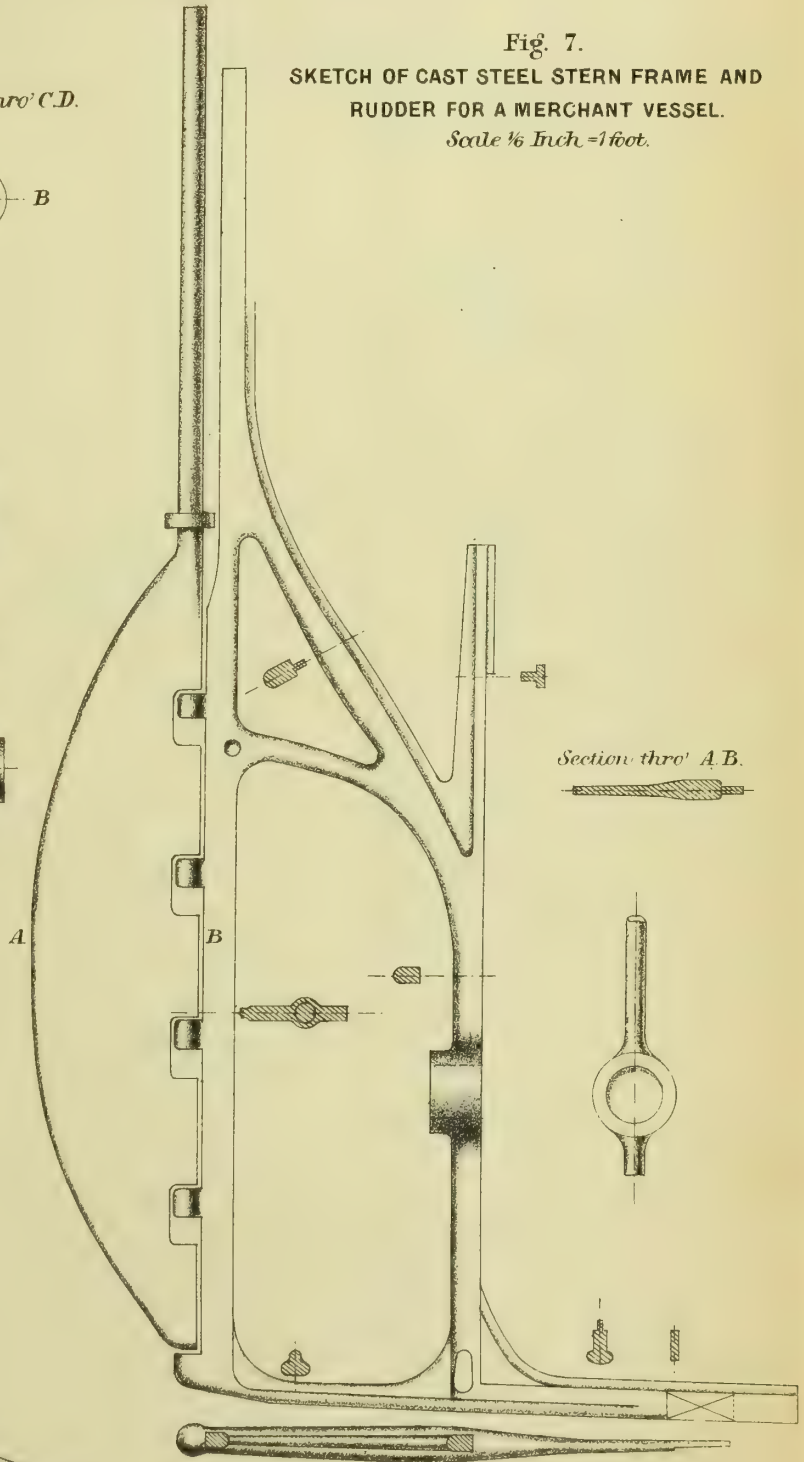


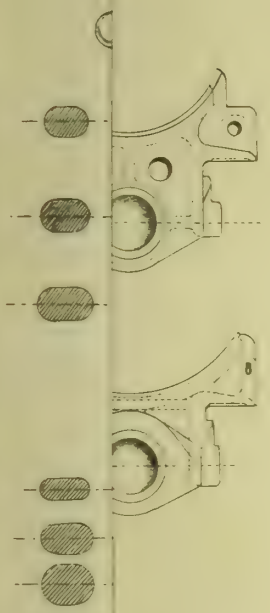
Fig. 7.
SKETCH OF CAST STEEL STERN FRAME AND RUDDER FOR A MERCHANT VESSEL.
Scale ⅛ Inch = 1 foot.



of Iron Forgings and Brass Castings.

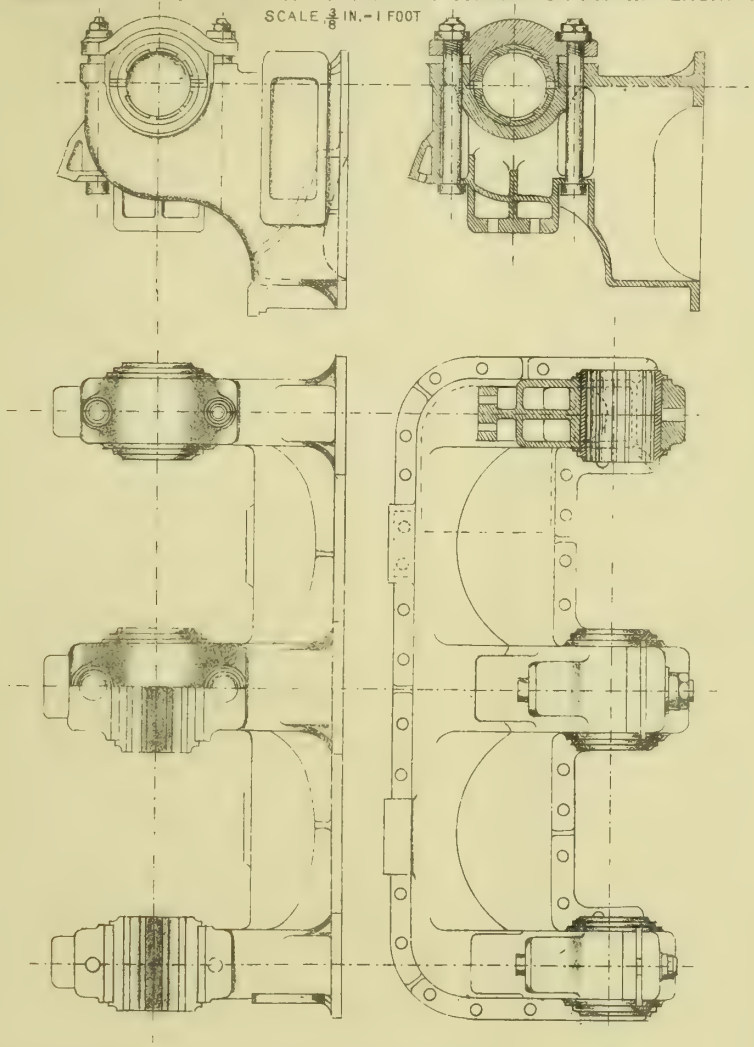
FIG. 10.

UN CARRIAGE.



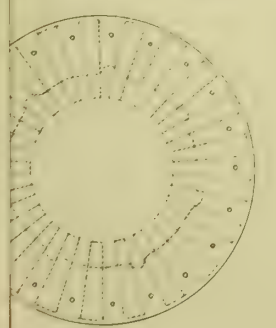
SKETCH OF CAST STEEL MAIN BEARING FRAME FOR MARINE ENGINE. FIG 11

SCALE $\frac{3}{8}$ IN. = 1 FOOT



SKETCH

CAPSTAN BODY



SKETCH OF CAST STEEL CAPSTAN BODY AND BAR HEAD. FIG. 14.

SCALE $\frac{3}{8}$ INCH = ONE FOOT.

BAR HEAD.

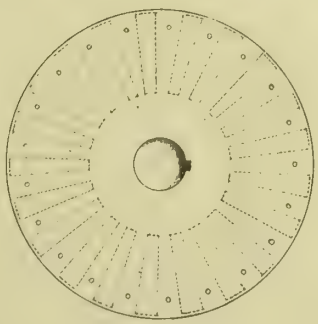


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FIG. 9.

SKETCH OF CAST STEEL ANCHOR.

SCALE, $\frac{1}{2}$ INCH = ONE FOOT.

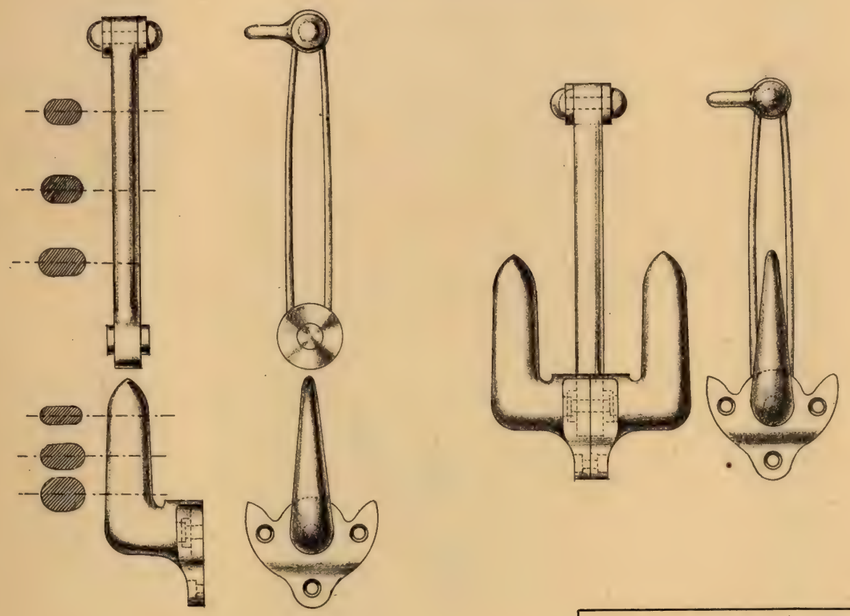
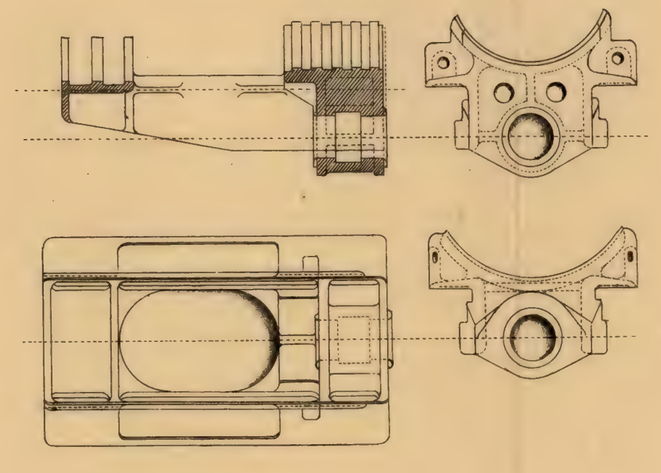


FIG. 10.

SKETCH OF CAST STEEL SADDLE FOR GUN CARRIAGE.

SCALE, $\frac{1}{4}$ INCH = ONE FOOT.



SKETCH OF CAST STEEL MAIN BEARING FRAME FOR MARINE ENGINE. FIG. 11

SCALE $\frac{3}{8}$ IN. = 1 FOOT.

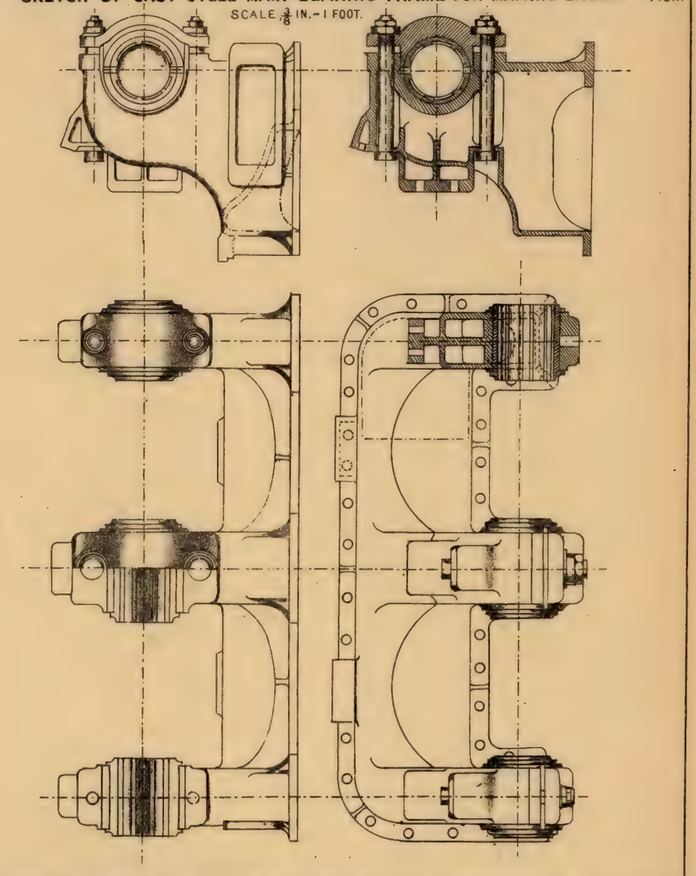


FIG. 12.

SKETCH OF CAST STEEL CROWN WHEEL OF CAPSTAN.

SCALE, $\frac{1}{4}$ INCH = ONE FOOT.

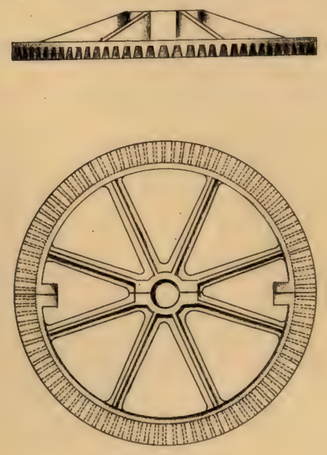
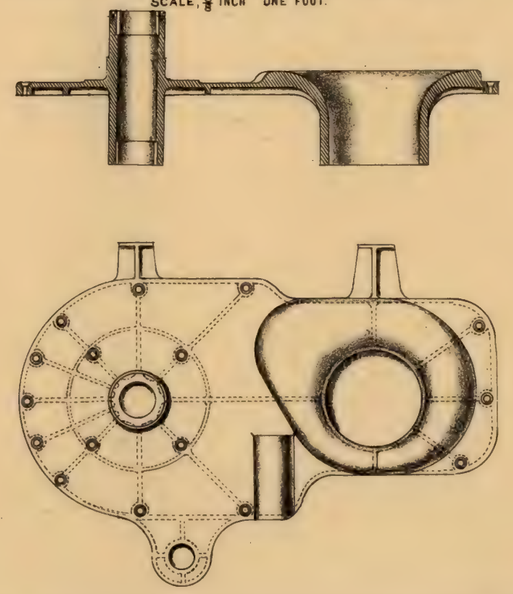


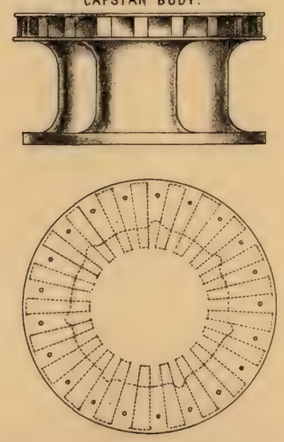
FIG. 13.

SKETCH OF CAST STEEL DECK PLATE, AND PIPE.

SCALE, $\frac{3}{8}$ INCH ONE FOOT.



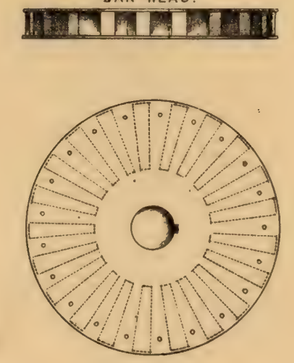
CAPSTAN BODY.



SKETCH OF CAST STEEL CAPSTAN BODY AND BAR HEAD. FIG. 14.

SCALE $\frac{3}{8}$ INCH = ONE FOOT.

BAR HEAD.



To Illustrate Mr. R. Sennett's Paper on Closed Stoke-holds.

H. M. S. "RODNEY."

FIG. 1.

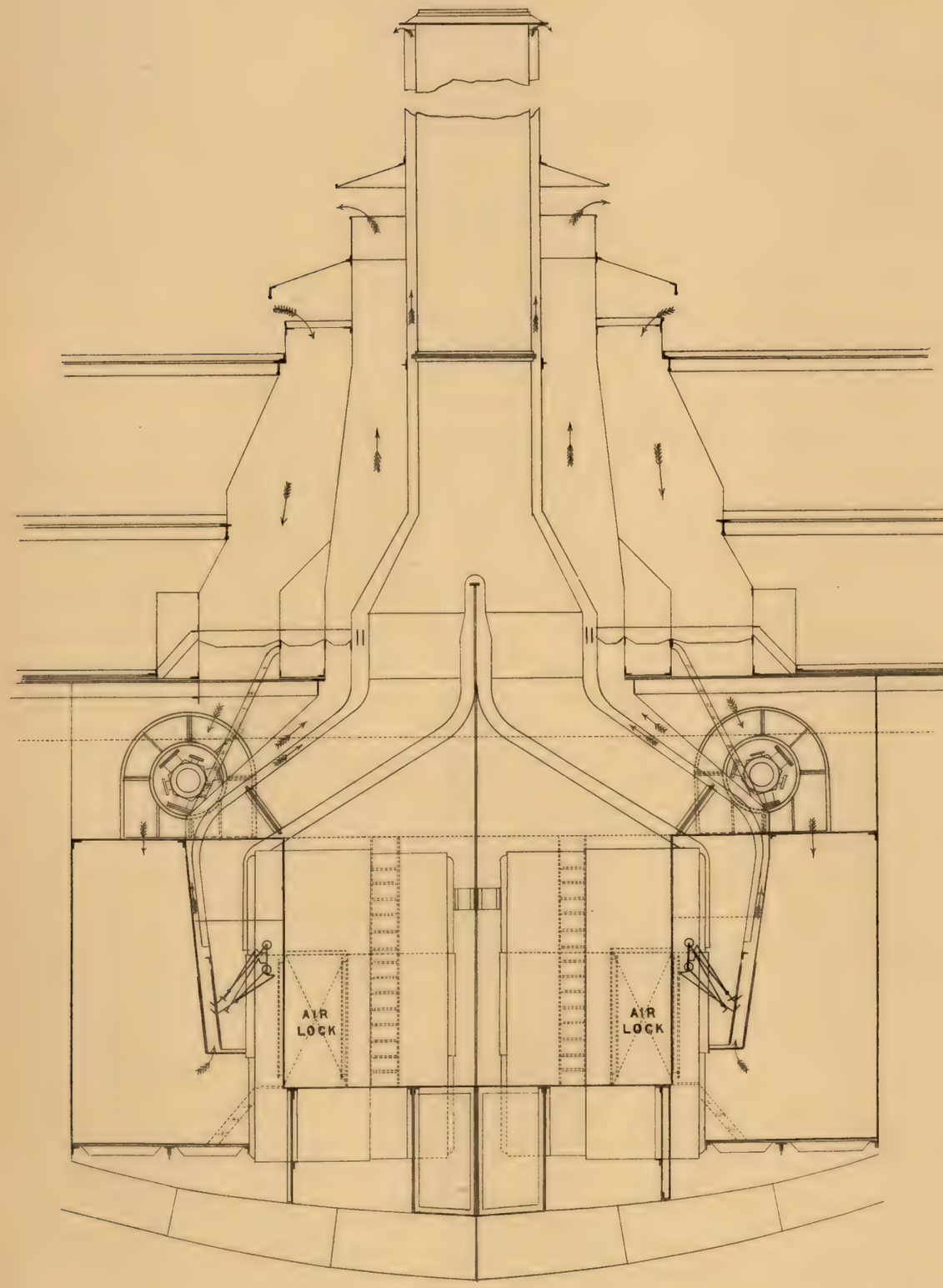
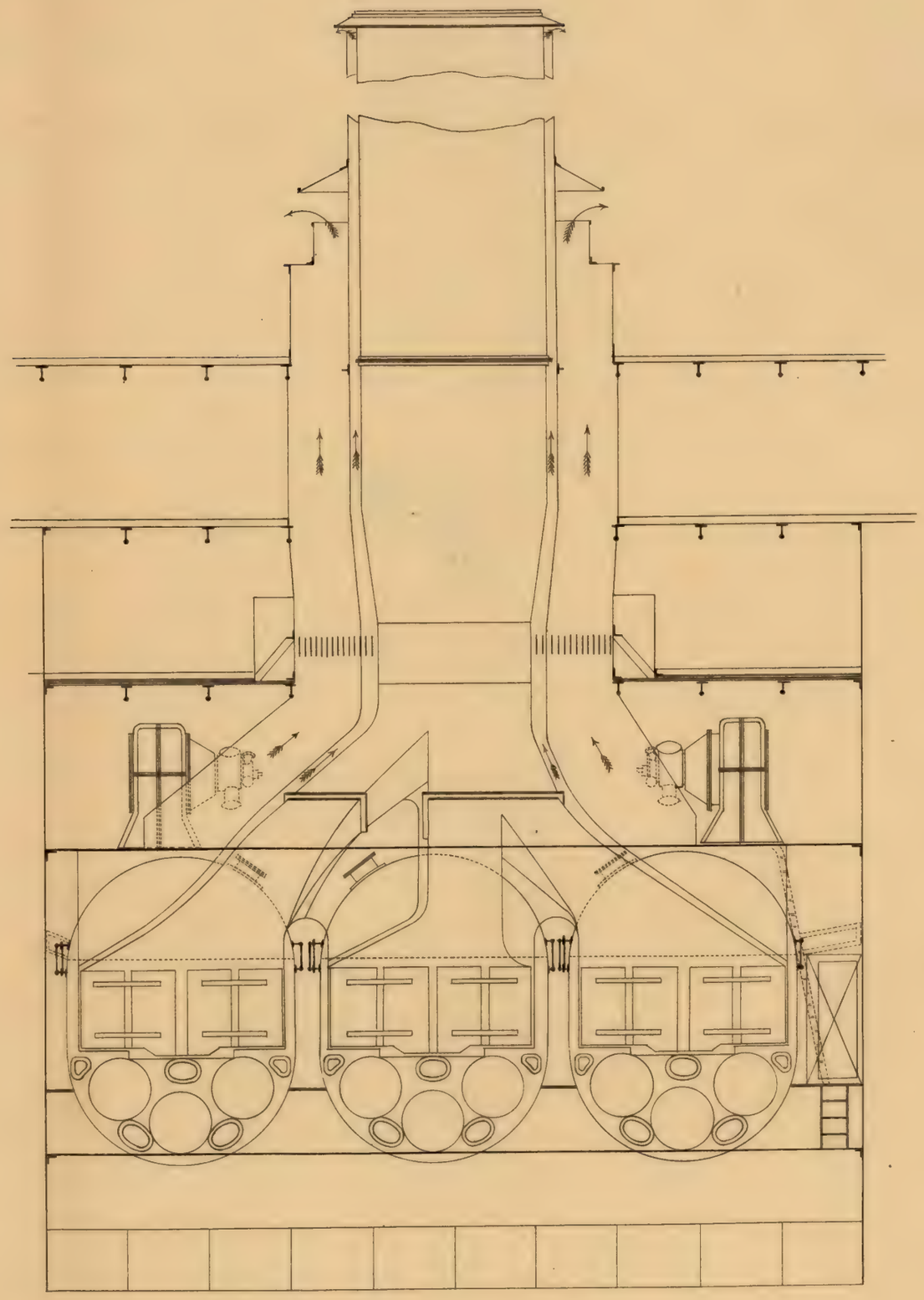
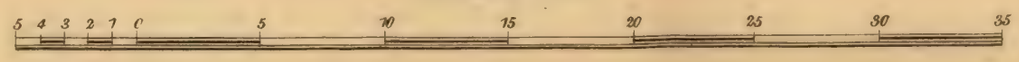


FIG. 2.

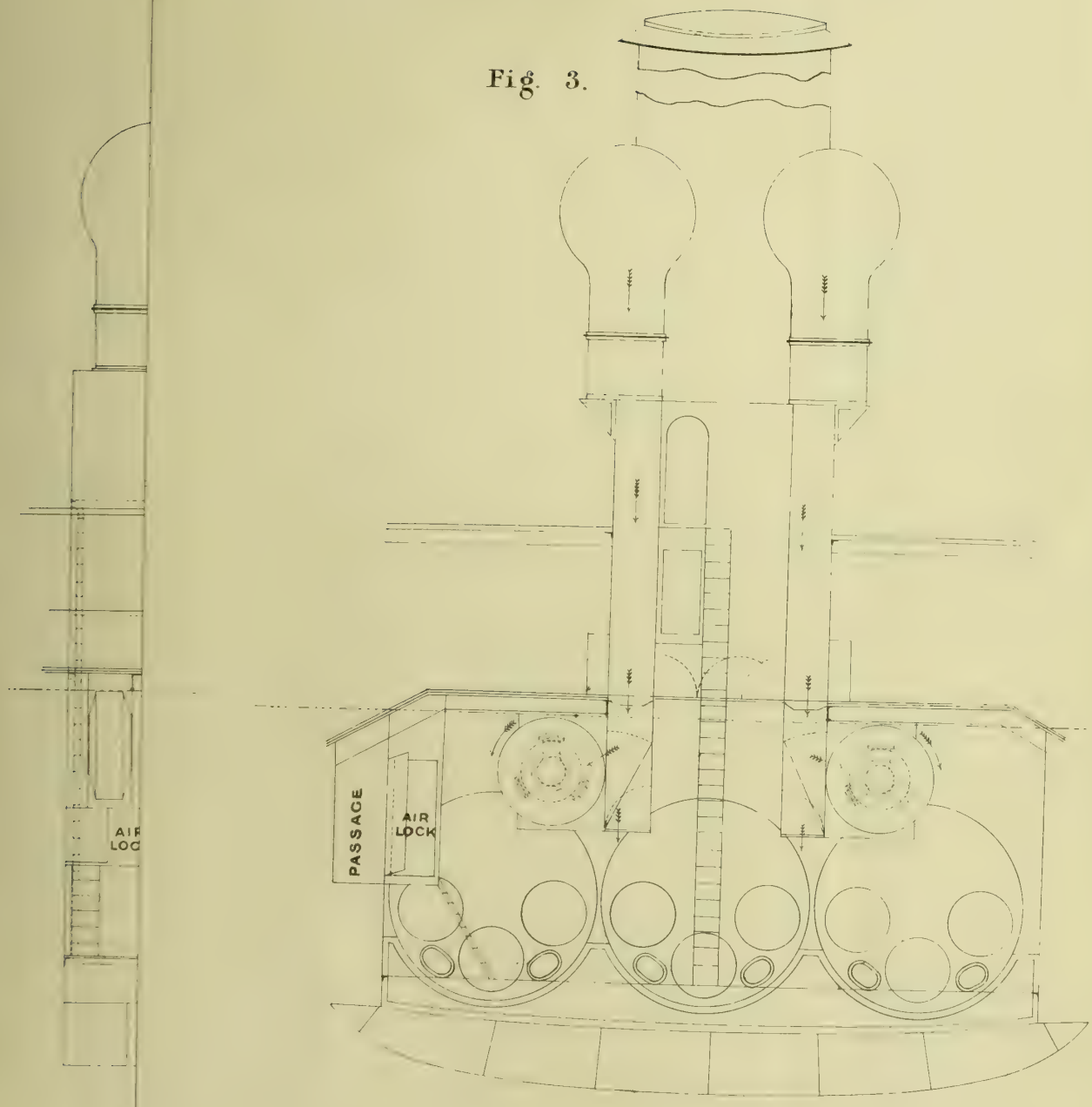


SCALE OF FEET



ke-holds.

Fig. 3.



To Illustrate Mr. R. Sennett's Paper on Closed Stoke-holds.

H. M. S. "MERSEY"

Fig. 4.

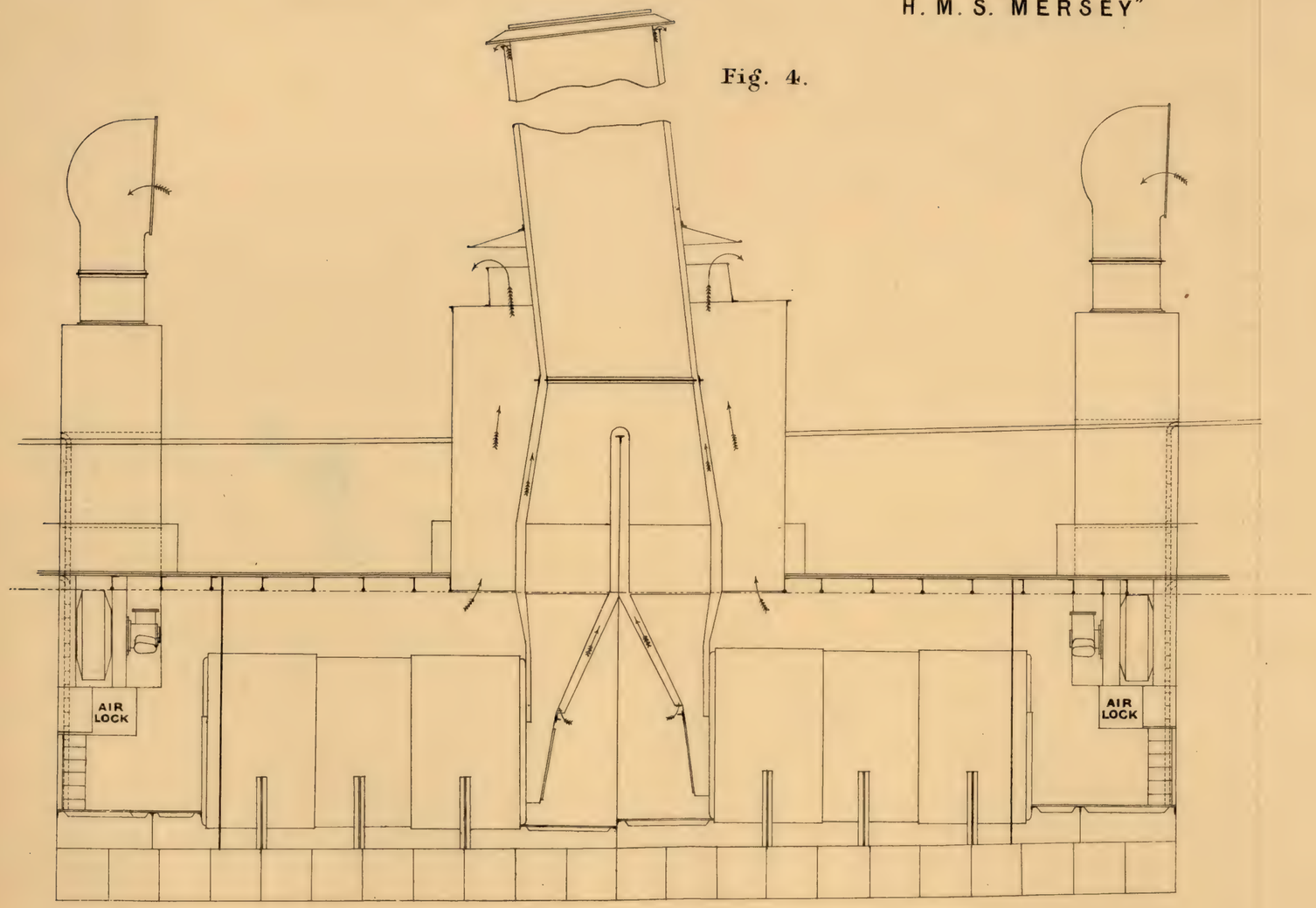
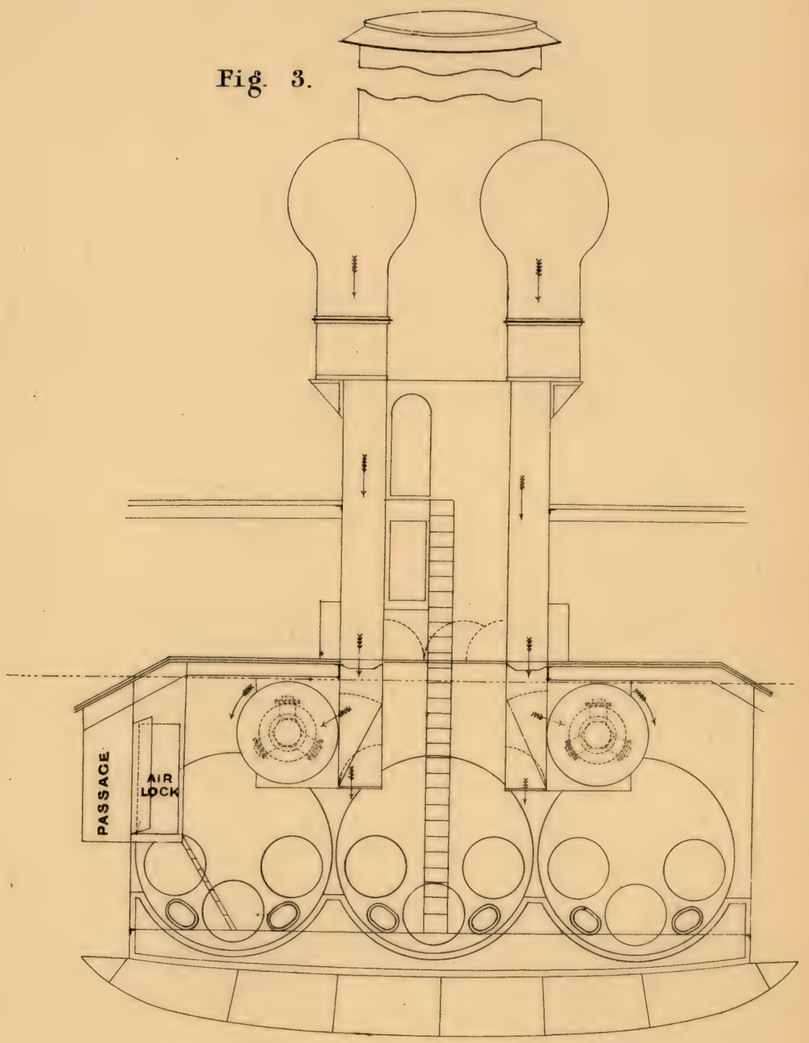


Fig. 3.



SCALE OF FEET.

To Illustrate Mr. J. Howden's Paper on forced Combustion in Furnaces of Steam Boilers.

FIG. 1.

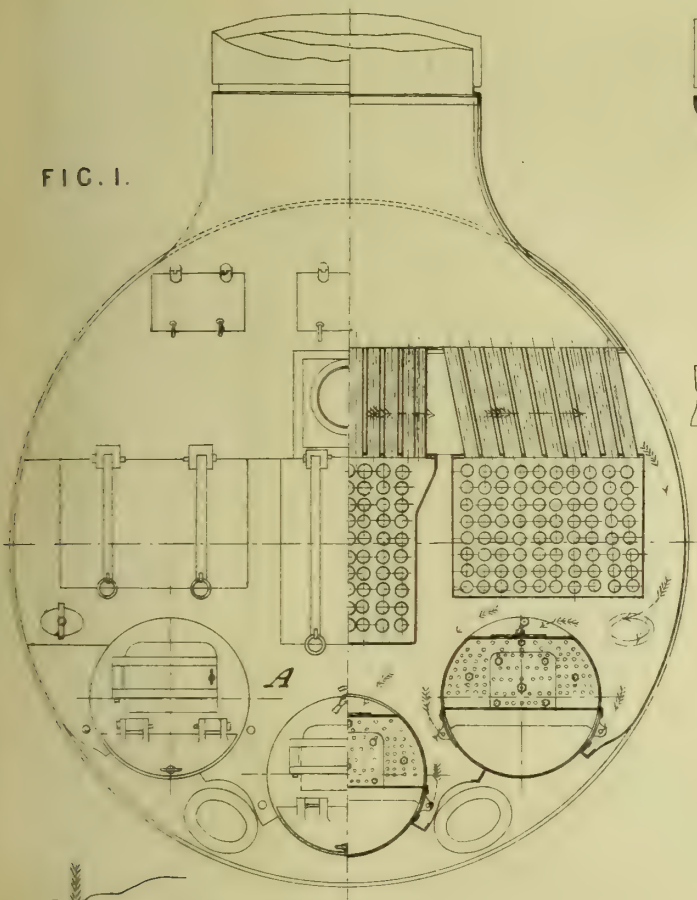


FIG. 2.

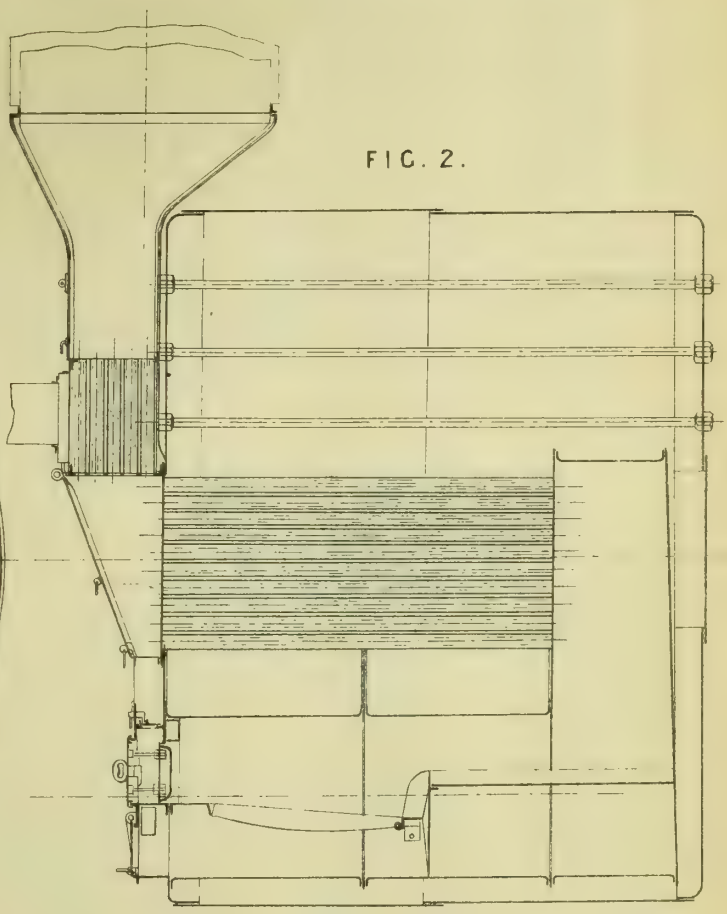
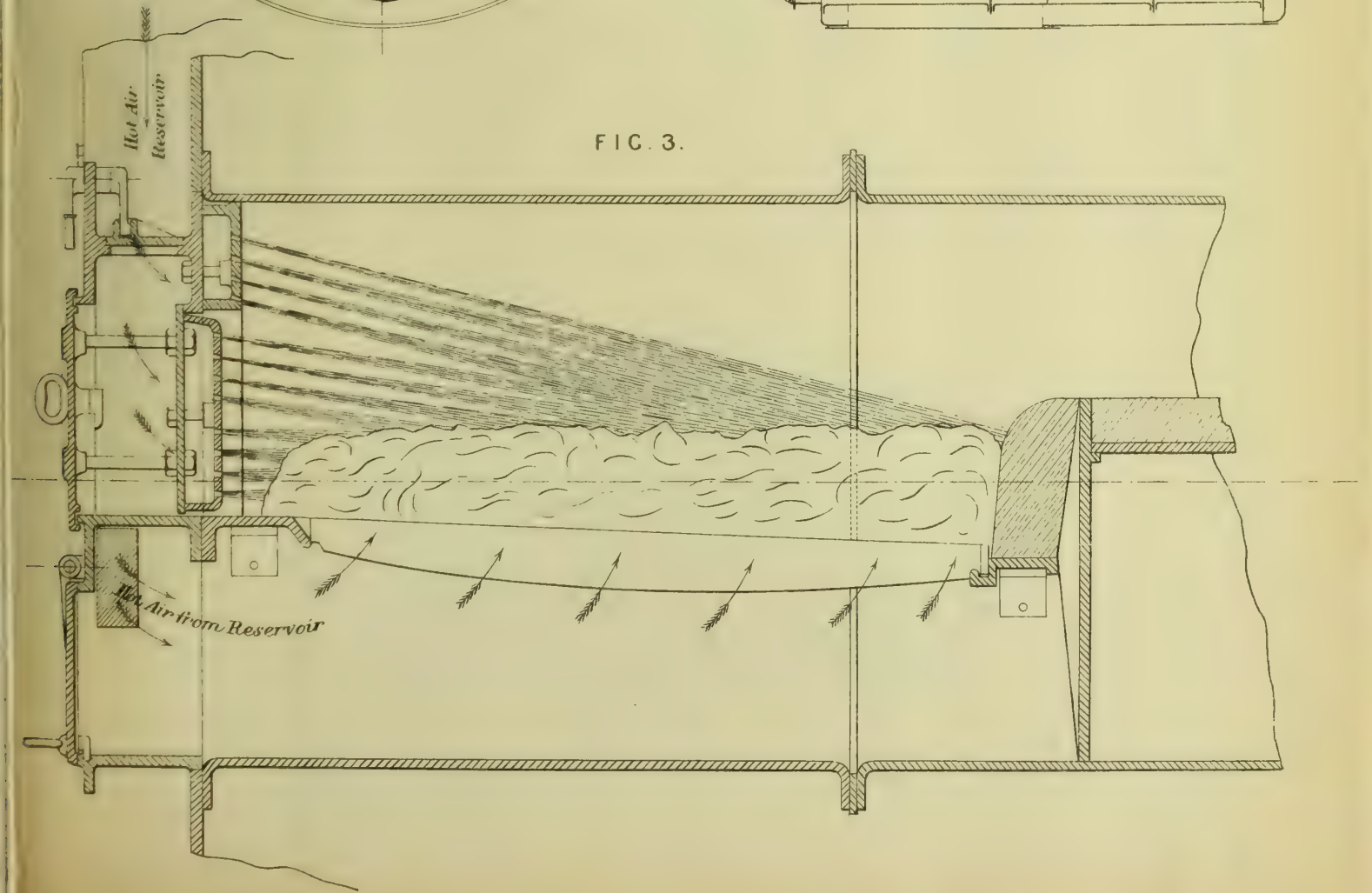
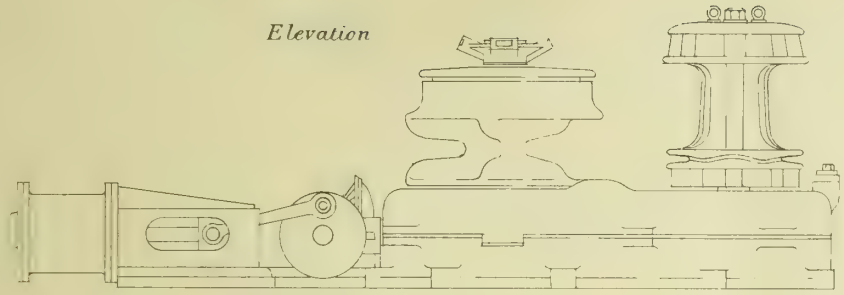


FIG. 3.

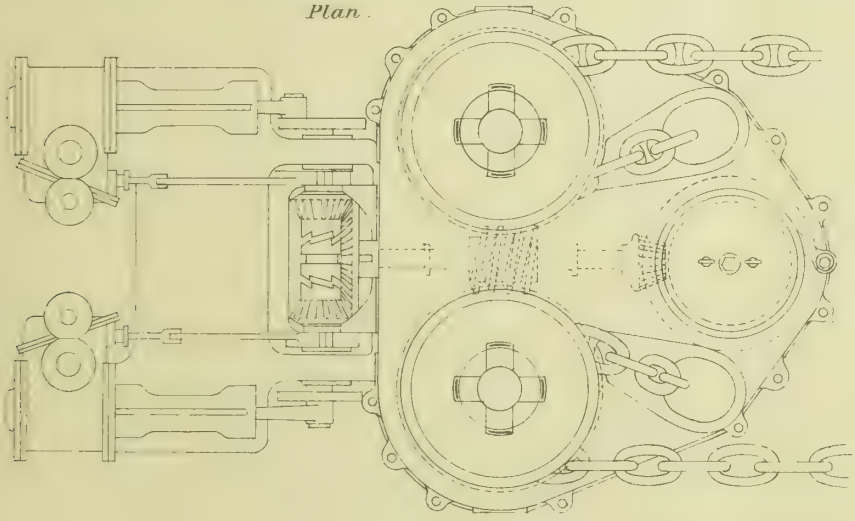


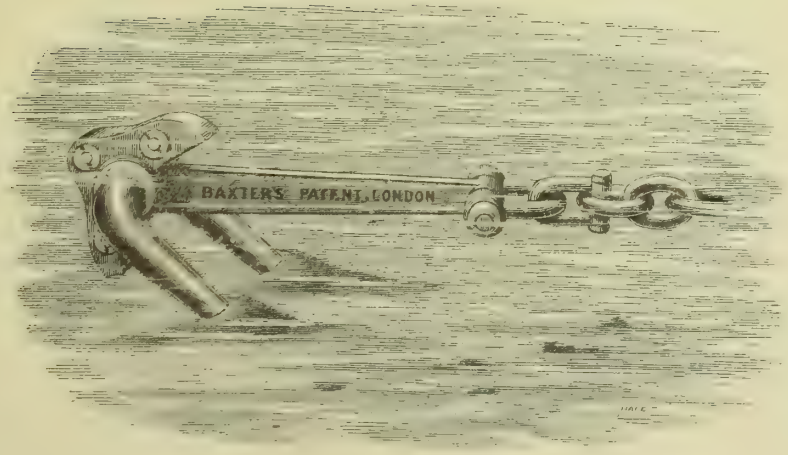
VERTICAL WINDLASS.

Elevation



Plan

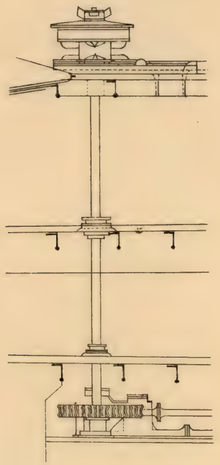




To Illustrate Mr. S. Baxter's Paper on Modern Improvements in the Working of Chain Cables and the Stowage of Anchors.

VERTICAL ARRANGEMENT.

Fig. 1.



HORIZONTAL ARRANGEMENT

Fig. 4.

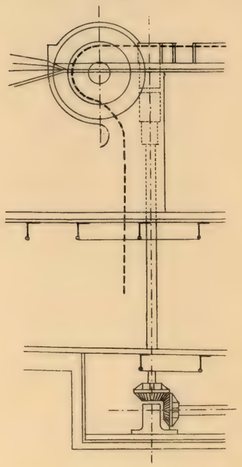


Fig. 2.

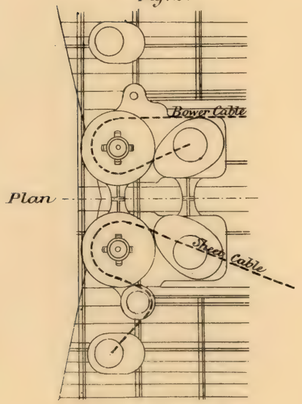


Fig. 5.

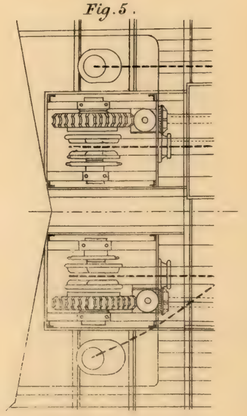
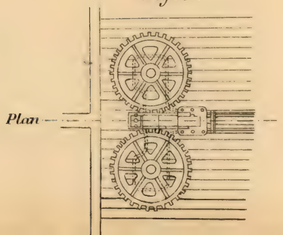
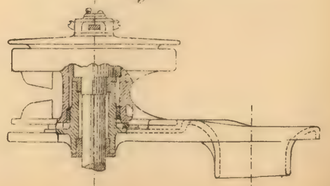


Fig. 3.



Part Section of Vertical Cable Holder

Fig. 6.



ELEVATION ANCHOR SEATING.

Fig. 7.

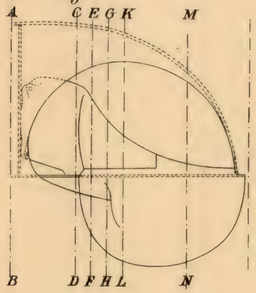


Fig. 9.
Section at A.B.

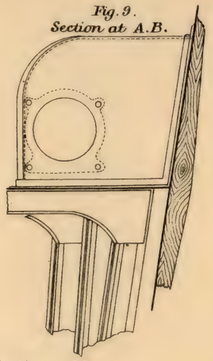
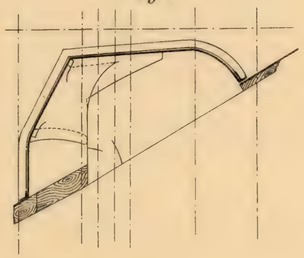
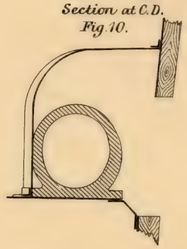


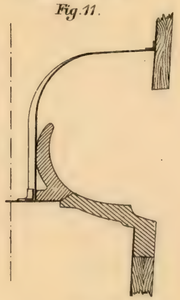
Fig. 8.



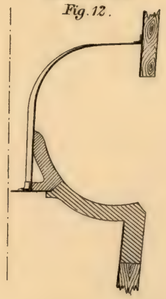
Section at C.D.
Fig. 10.



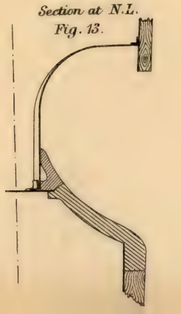
Section at E.F.
Fig. 11.



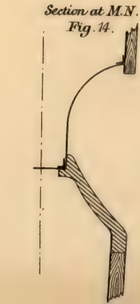
Section at G.H.
Fig. 12.



Section at N.L.
Fig. 13.

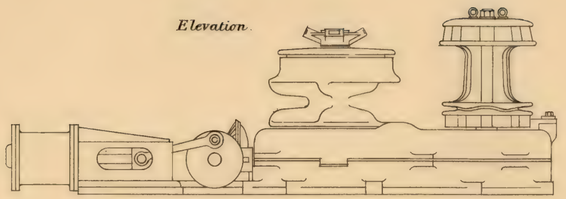


Section at M.N.
Fig. 14.

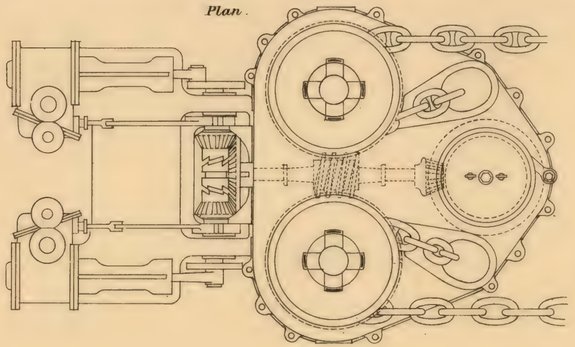


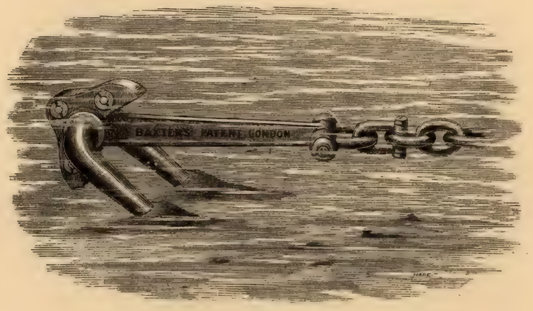
VERTICAL WINDLASS.

Elevation.



Plan.



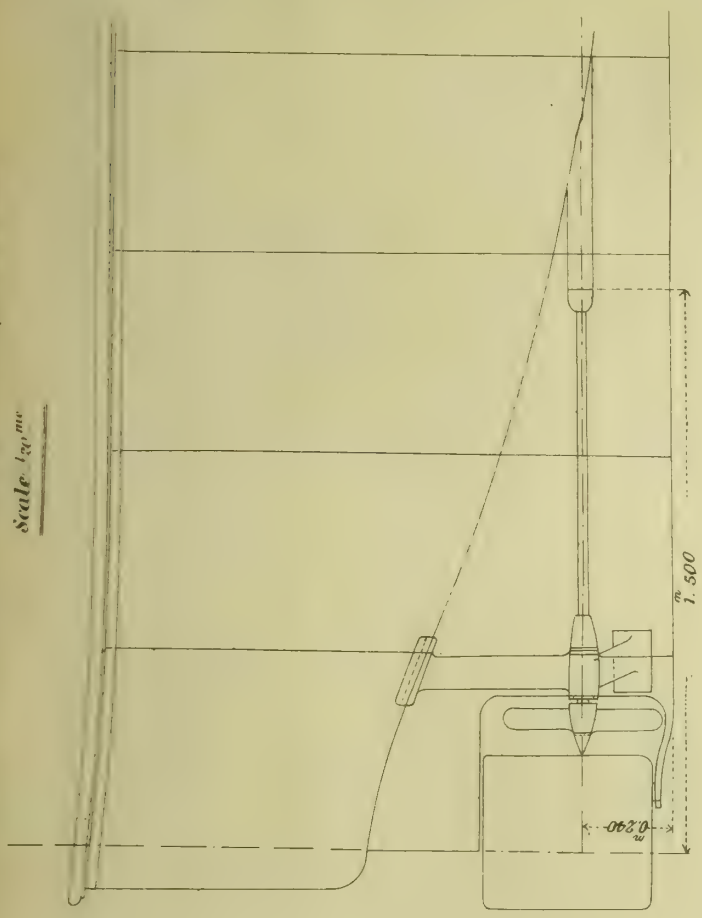


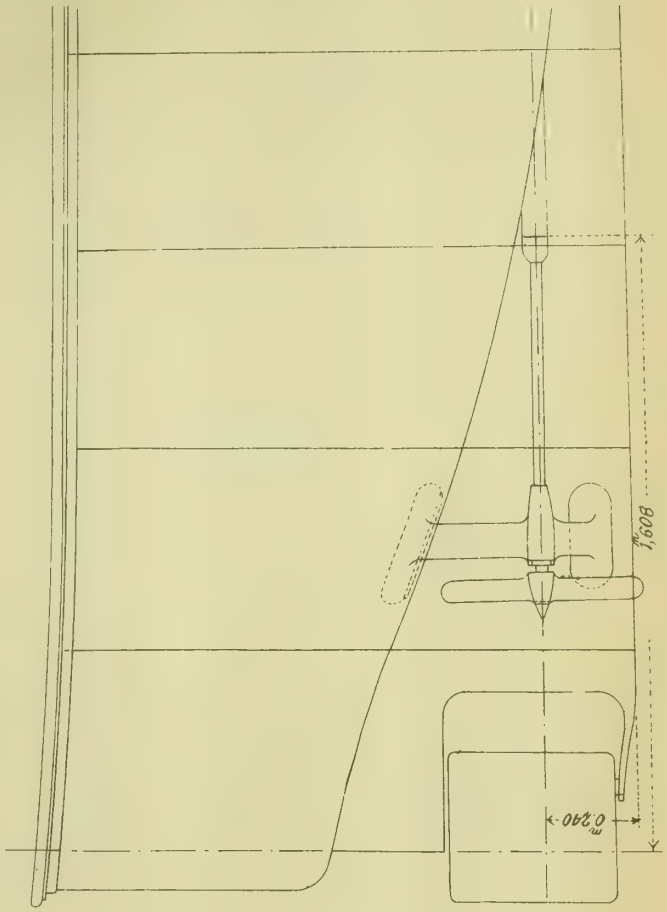
To Illustrate Mons. M. Marchal's Paper on Experience in Propulsion with Three Screws.

3 SCREW STEAM LAUNCH WITH 3 SCREWS.

Position of the screws in the different arrangements tested.

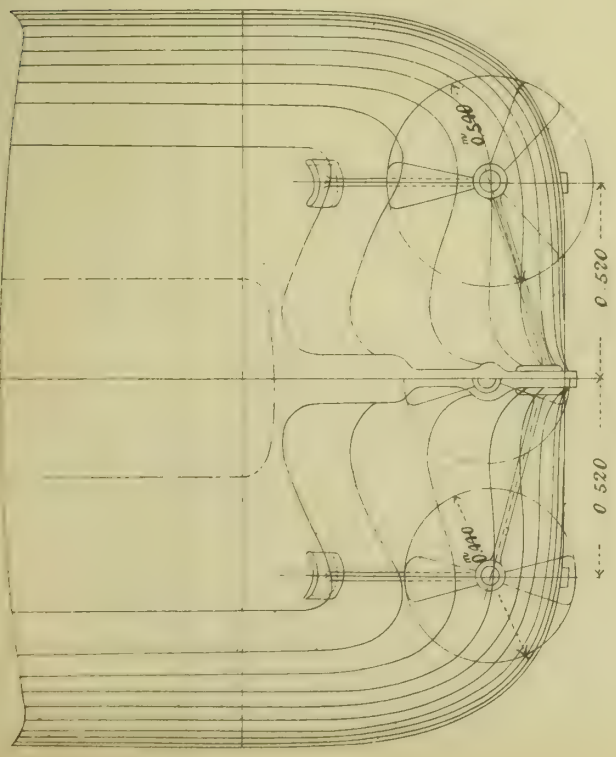
Scale 1/20 me





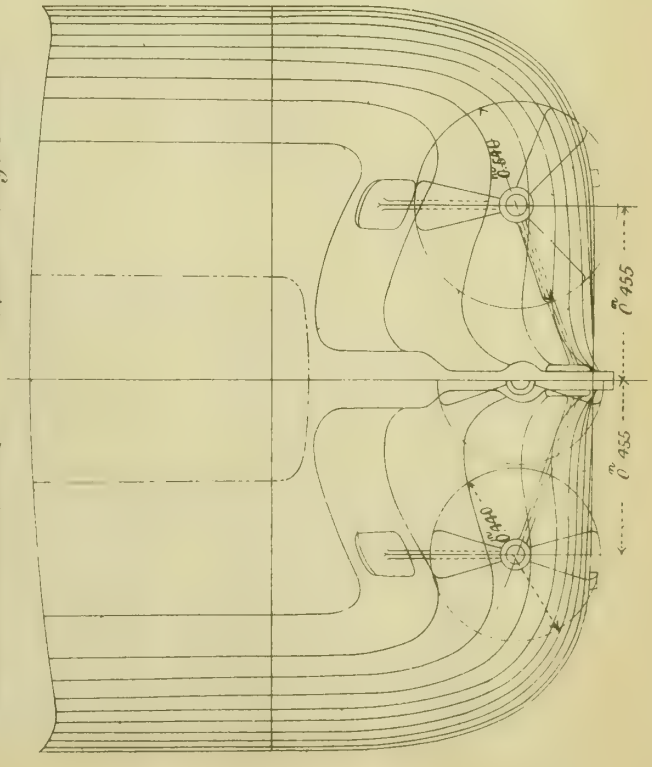
1ST SERIES OF EXPERIMENTS.

Case of 3 small screws *Case of 2 large screws*

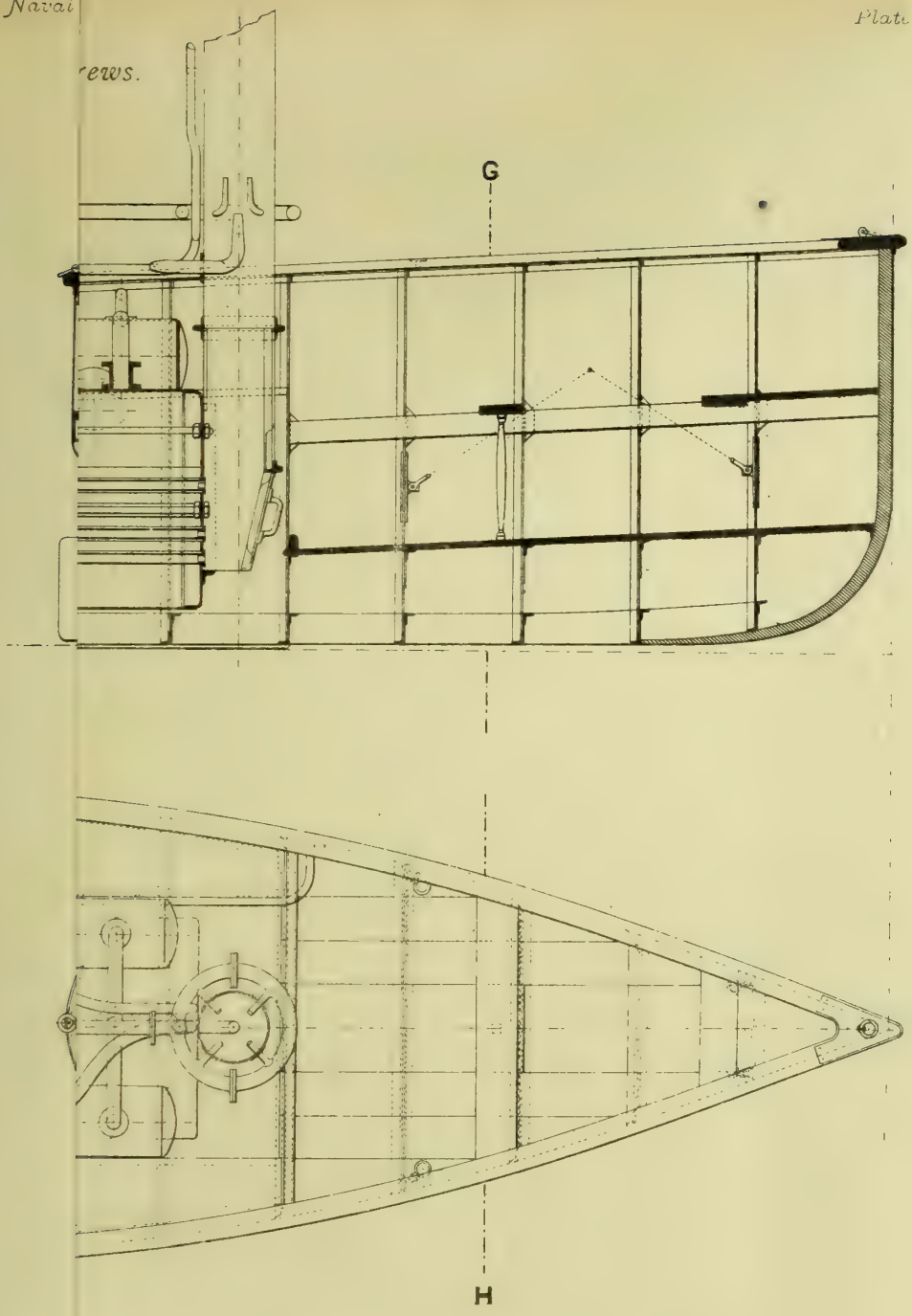


2ND SERIES OF EXPERIMENTS.

Case of 3 small screws. *Case of 2 large screws.*



rews.



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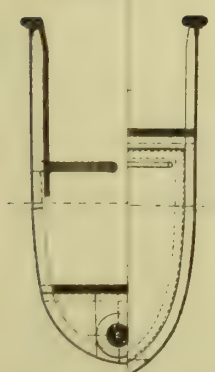
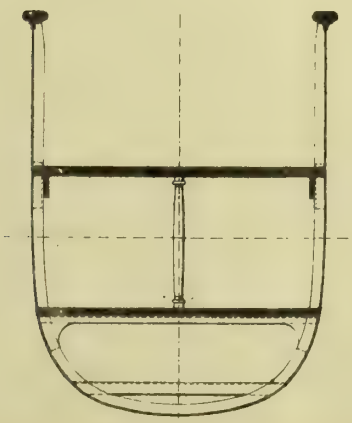
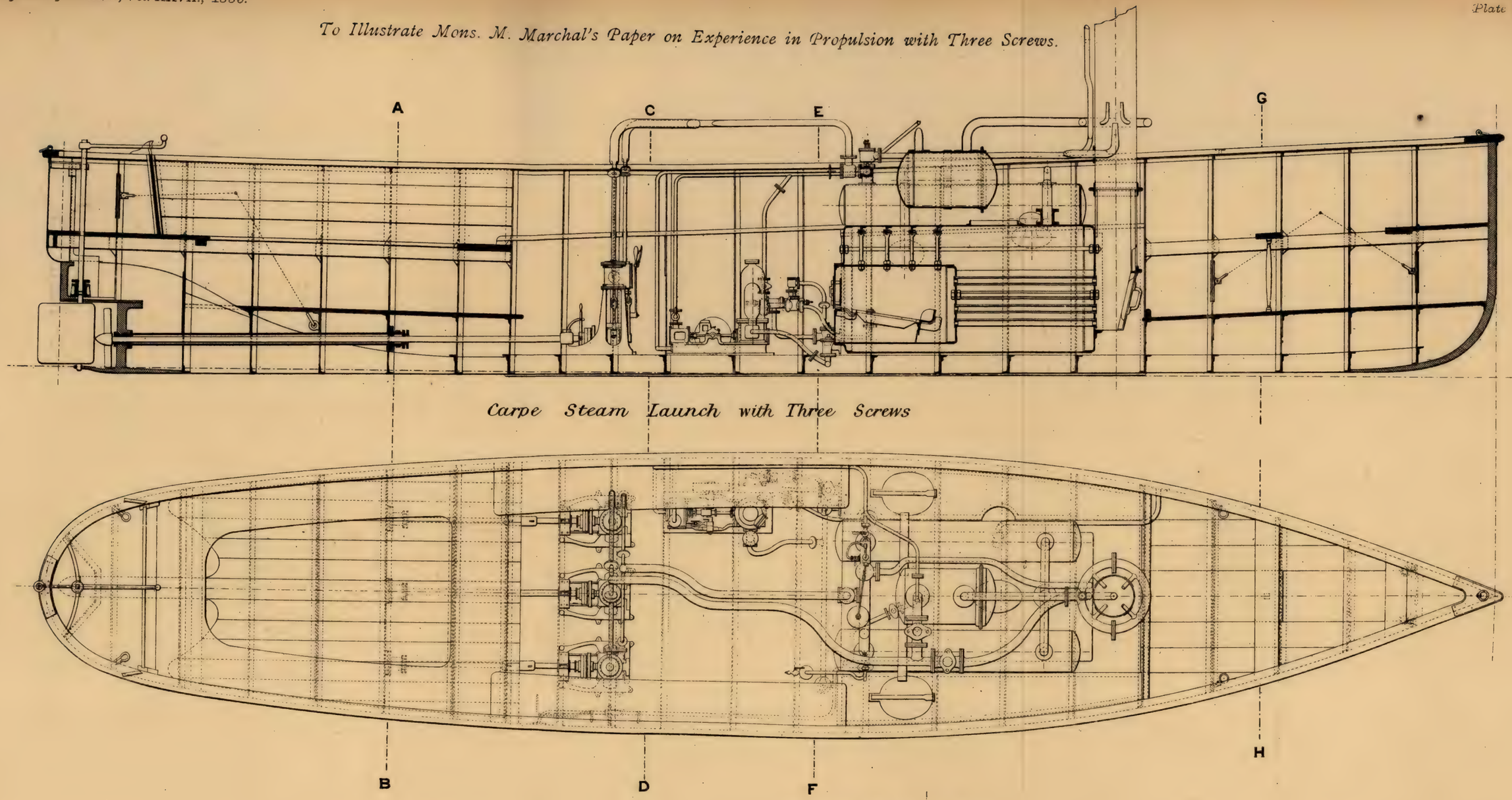


Section at G. H.



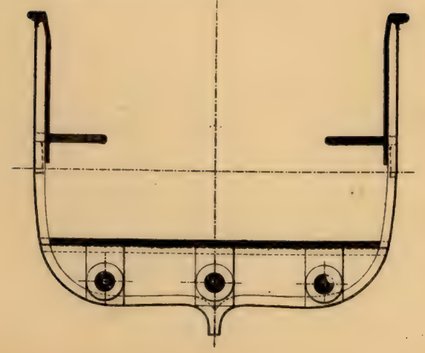
Scale - $\frac{1}{30}$

To Illustrate Mons. M. Marchal's Paper on Experience in Propulsion with Three Screws.

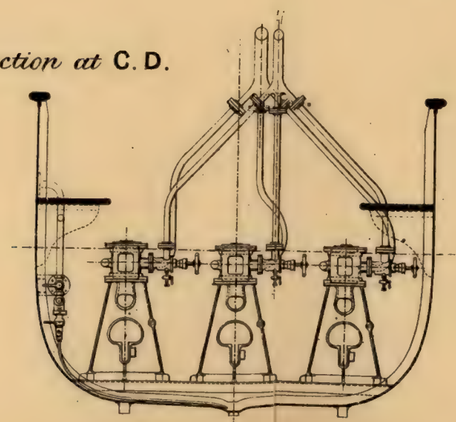


Carpe Steam Launch with Three Screws

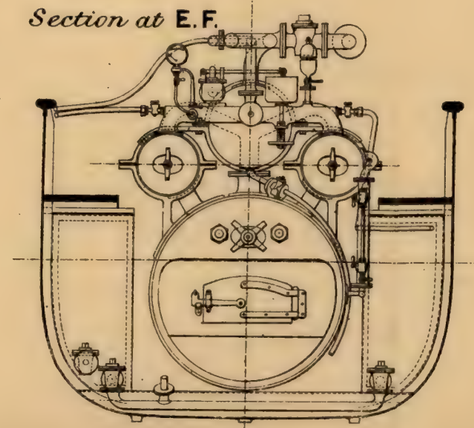
Section at A.B.



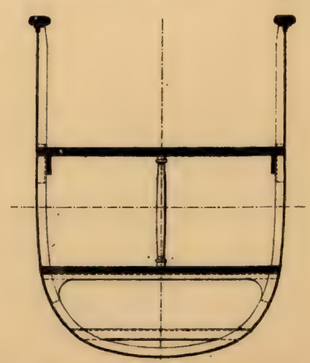
Section at C.D.



Section at E.F.



Section at G.H.



Scale – $\frac{1}{30}$

lsion with Three Screws.

alues
et 3.

	7½
1	24.550
5	23.520
5	22.225
1	19.200
2	18.825

ues of

	7½
2	
5	
2	

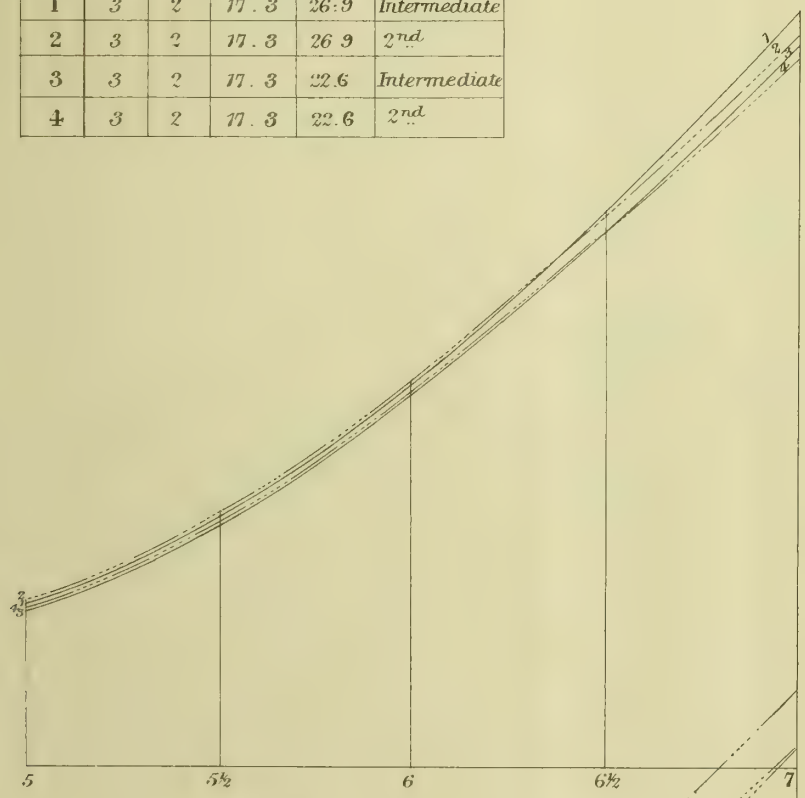
n the

6,k.875
2,k.867
ne H.P. 3,92

Sheet C.
TRIALS OF THE "CARPE."

Comparison of the results obtained with the small pitch triple Screws and the large pitch triple Screws of the 2nd, and Intermediate Series.

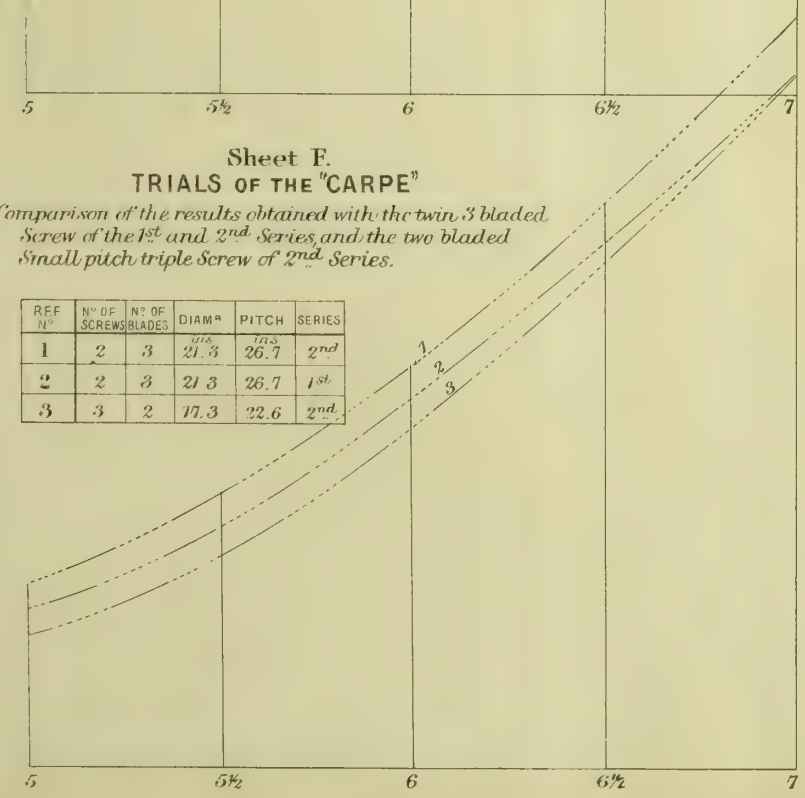
REF. N°	N° OF SCREWS	N° OF BLADES	DIAMETER	PITCH	SERIES
			ins	ins	
1	3	2	17.3	26:9	Intermediate
2	3	2	17.3	26.9	2nd
3	3	2	17.3	22.6	Intermediate
4	3	2	17.3	22.6	2nd



Sheet F.
TRIALS OF THE "CARPE"

Comparison of the results obtained with the twin 3 bladed Screw of the 1st and 2nd Series, and the two bladed Small pitch triple Screw of 2nd Series.

REF N°	N° OF SCREWS	N° OF BLADES	DIAMR	PITCH	SERIES
			ins	ins	
1	2	3	21.3	26.7	2nd
2	2	3	21.3	26.7	1st
3	3	2	17.3	22.6	2nd

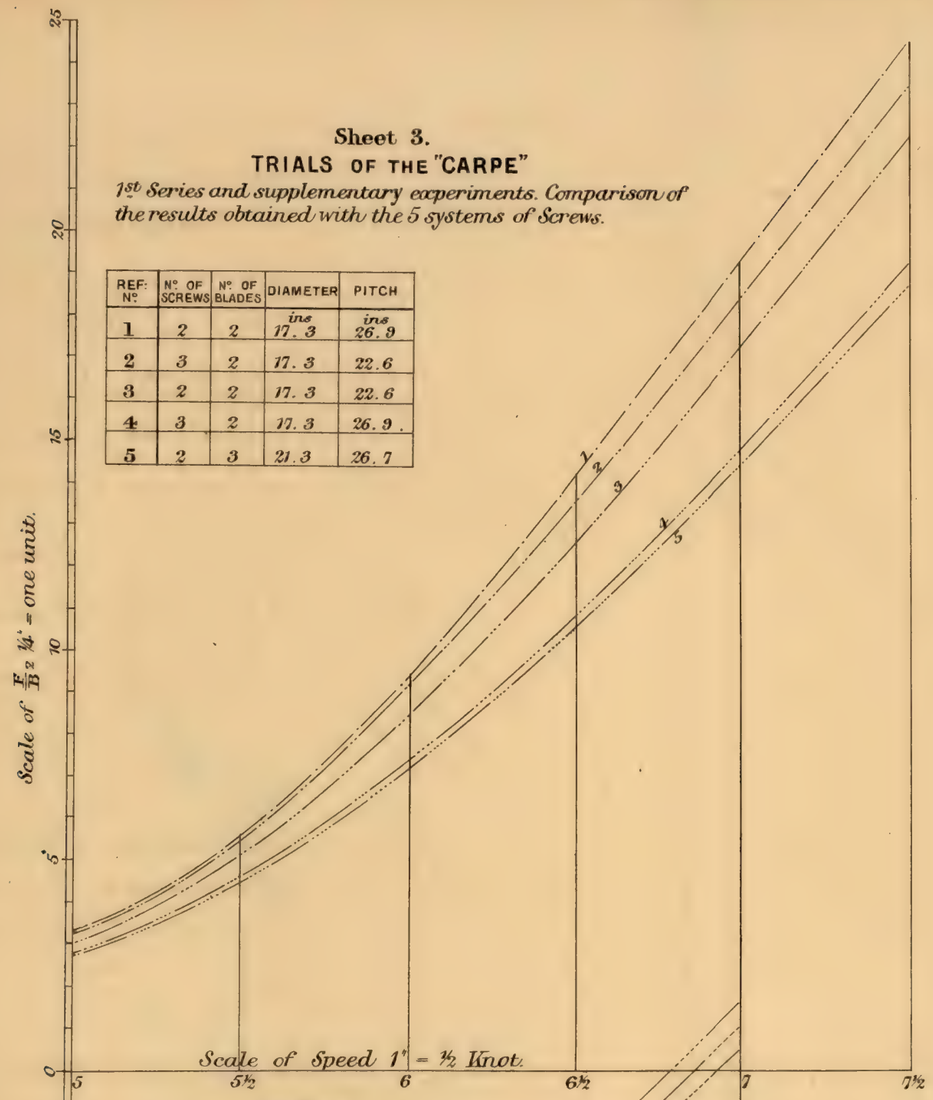


To Illustrate Mons. M. Marchal's Paper on Experience in Propulsion with Three Screws.

Sheet 3.
TRIALS OF THE "CARPE"

1st Series and supplementary experiments. Comparison of the results obtained with the 5 systems of Screws.

REF: Nº	Nº OF SCREWS	Nº OF BLADES	DIAMETER	PITCH
			ins	ins
1	2	2	17.3	26.9
2	3	2	17.3	22.6
3	2	2	17.3	22.6
4	3	2	17.3	26.9
5	2	3	21.3	26.7



Sheet 4.
TRIALS OF THE "CARPE". 2ND SERIES.

Comparison of the results obtained with the three systems tried in 2nd Series.

REF: Nº	Nº OF SCREWS	Nº OF BLADES	DIAMETER	PITCH
			ins.	ins.
1	2	3	21.3	26.7
2	3	2	17.3	26.7
3	3	2	17.3	22.6

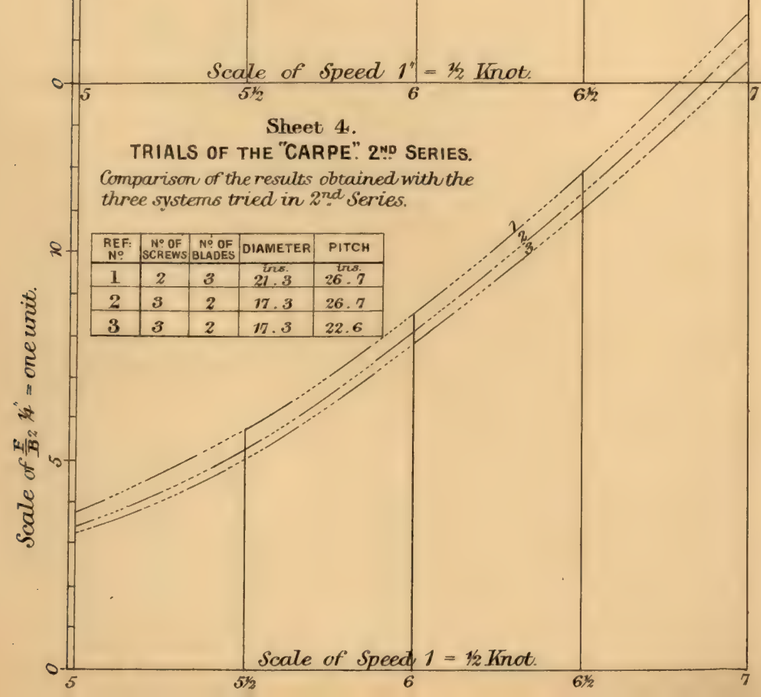


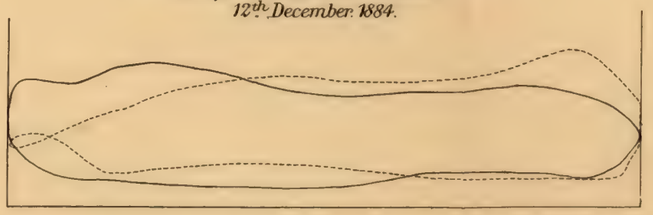
Table *shewing the comparative values of* $\frac{F}{B^2}$ *for the various screws in Sheet 3.*

Reference Nº	Knots.					
	5	5½	6	6½	7	7½
1	3.377	5.650	9.425	14.200	19.400	24.550
2	3.300	5.585	9.185	13.585	18.385	23.520
3	3.062	5.175	8.550	12.551	17.325	22.225
4	2.833	4.612	7.425	10.875	14.814	19.200
5	2.750	4.450	7.178	10.625	14.500	18.825

Table *shewing the comparative values of* $\frac{F}{B^2}$ *for the various screws in Sheet 4.*

Reference Nº	Knots.					
	5	5½	6	6½	7	7½
1		5.800	8.525	11.810	15.710	
2		5.350	8.085	11.450	15.175	
3		5.135	7.850	11.085	14.612	

Specimen of a diagram taken from the Carpe at 680 Revolutions.
12th December 1884.

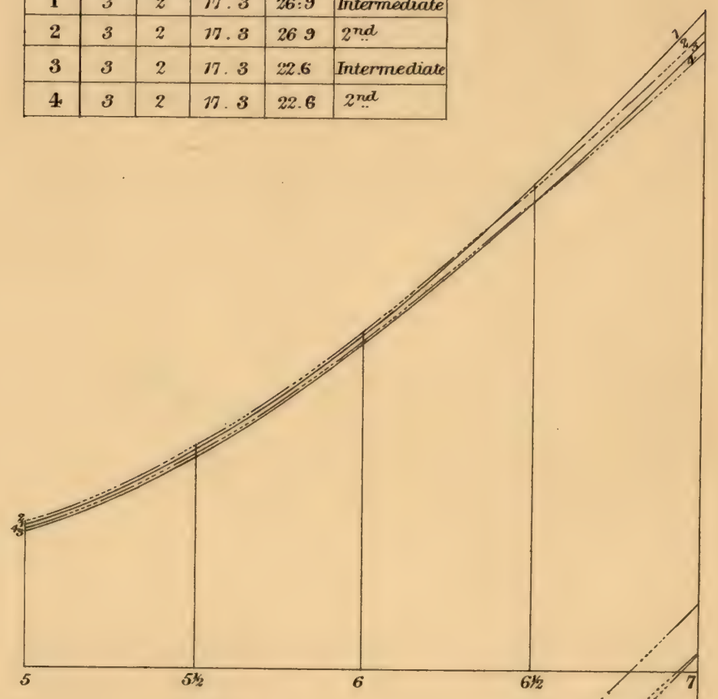


Pressure in Steam pipe	*6,k875*
Mean pressure on piston	*2,k867*
Indicated power N.B. 75 Kilog m/m One H.P.	*9,92*

Sheet C.
TRIALS OF THE "CARPE."

Comparison of the results obtained with the small pitch triple Screws and the large pitch triple Screws of the 2nd and Intermediate Series.

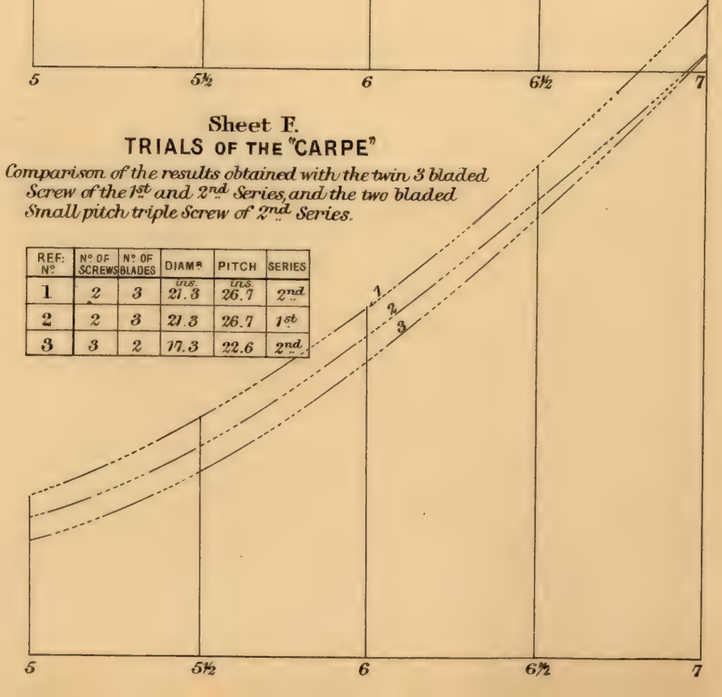
REF. Nº	Nº OF SCREWS	Nº OF BLADES	DIAMETER	PITCH	SERIES
			ins	ins	
1	3	2	17.3	26.9	Intermediate
2	3	2	17.3	26.9	2nd
3	3	2	17.3	22.6	Intermediate
4	3	2	17.3	22.6	2nd



Sheet F.
TRIALS OF THE "CARPE"

Comparison of the results obtained with the twin 3 bladed Screw of the 1st and 2nd Series, and the two bladed Small pitch triple Screw of 2nd Series.

REF: Nº	Nº OF SCREWS	Nº OF BLADES	DIAMR	PITCH	SERIES
			ins.	ins.	
1	2	3	21.3	26.7	2nd
2	2	3	21.3	26.7	1st
3	3	2	17.3	22.6	2nd



To Illustrate Mr. R. E. Froude's Paper on the Determination of the most suitable Dimensions for Screw Propellers.

FIG. 1.

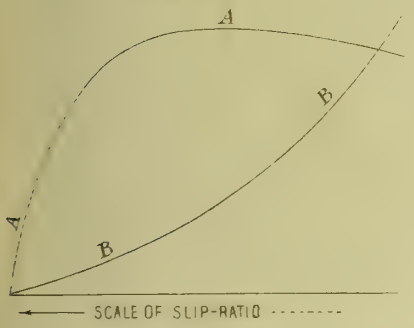


FIG. 2.

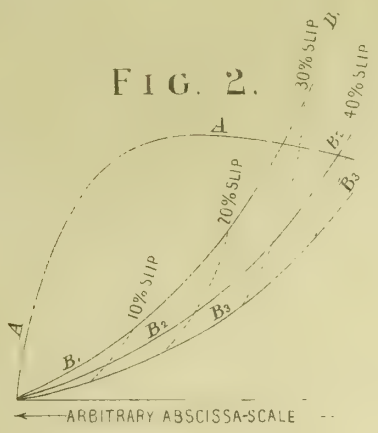
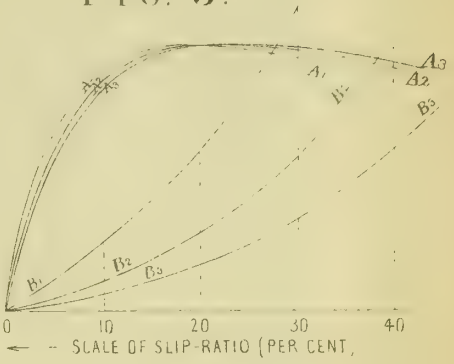


FIG. 3.



PROPULSIVE COEFFICIENTS i.e. VALUES OF $\frac{E.H.P.}{I.H.P.}$ FOR ACTUAL SHIPS, COMPARED WITH EFFICIENCY CURVE GIVEN BY MODEL SCREW-EXPERIMENTS

REFERENCES

+ 1	"IRIS"	Original 4 bladed screws	Feb 4 1878	Speed 16·6 knots	
+ 2		alternate blades removed	Feb 15 1878	15·7	"
+ 3		new reduced 4 bladed screws	July 3 1878	18·6	
+ 4	"	new 2 bladed screws	(Aug 1 1878)	18·6	"
+ 5		new 4 bladed screws load draft	June 12 1880	Speed 18·0 kn	
X 6	MERCURY		Aug 6 1879	Speed 18·9 kn	
X 7	PHAETON		(Feb 12 1884)	" 18·7	"
⊕ 8	COLLINGWOOD		(? date)	16·6	"
⊕ 9	"		(? date)	" 16·05	"
⊕ 10	"	Pitch reduced to 17·5 ft		16·8	"
⊕ 11	EDINBURGH		(Sept 18 1883)	Speed 16·0 knots	
⊗ 12	CONQUEROR		(date)	" 15·5	"
⊙ 13	DEVASTATION		(Oct 31 1872)	" 13·8	"
⊙ 14	"		(April 15 1873)	" 13·3	"
⊙ 15	THUNDERER		(Jan 4 1877)	" 13·4	"
○ 16	"		(Jan 6 1877)	13·0	"
◎ 17	INFLEXIBLE	2 bladed screws	(Nov 14 1878)	Speed 14·7 knots	
◎ 18		4 " "	(Dec 18 1880)	14·3	
◎ 19	"	" " "	(Nov 30 1881)	13·8	

X i.e. EHP OF SHIP WITH SHAFT-TUBES AND OTHER APPENDAGES

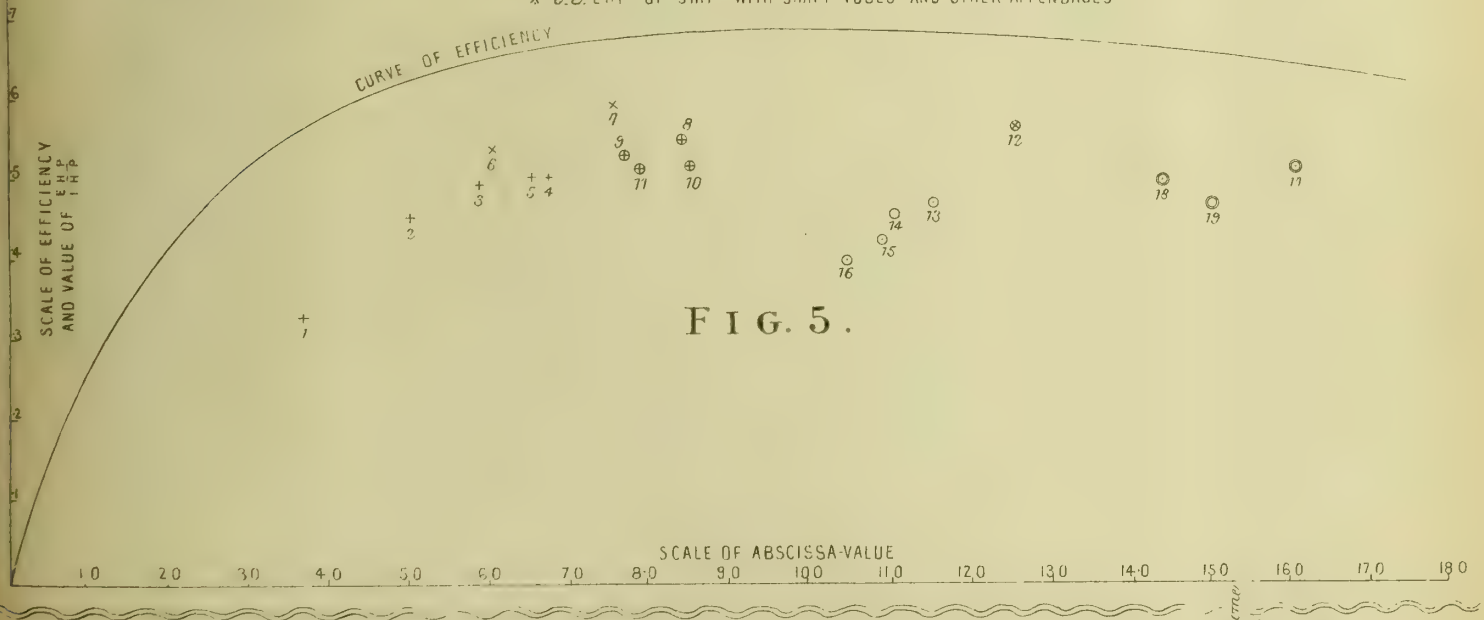
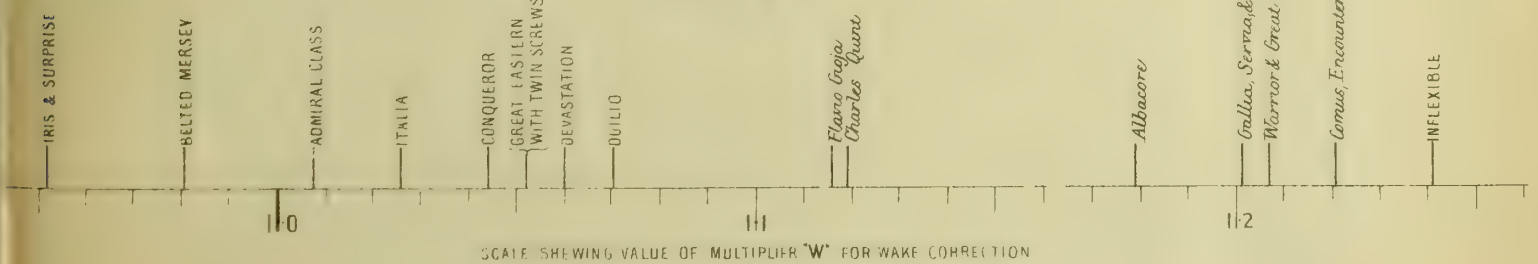


FIG. 5.

FIG. 8.

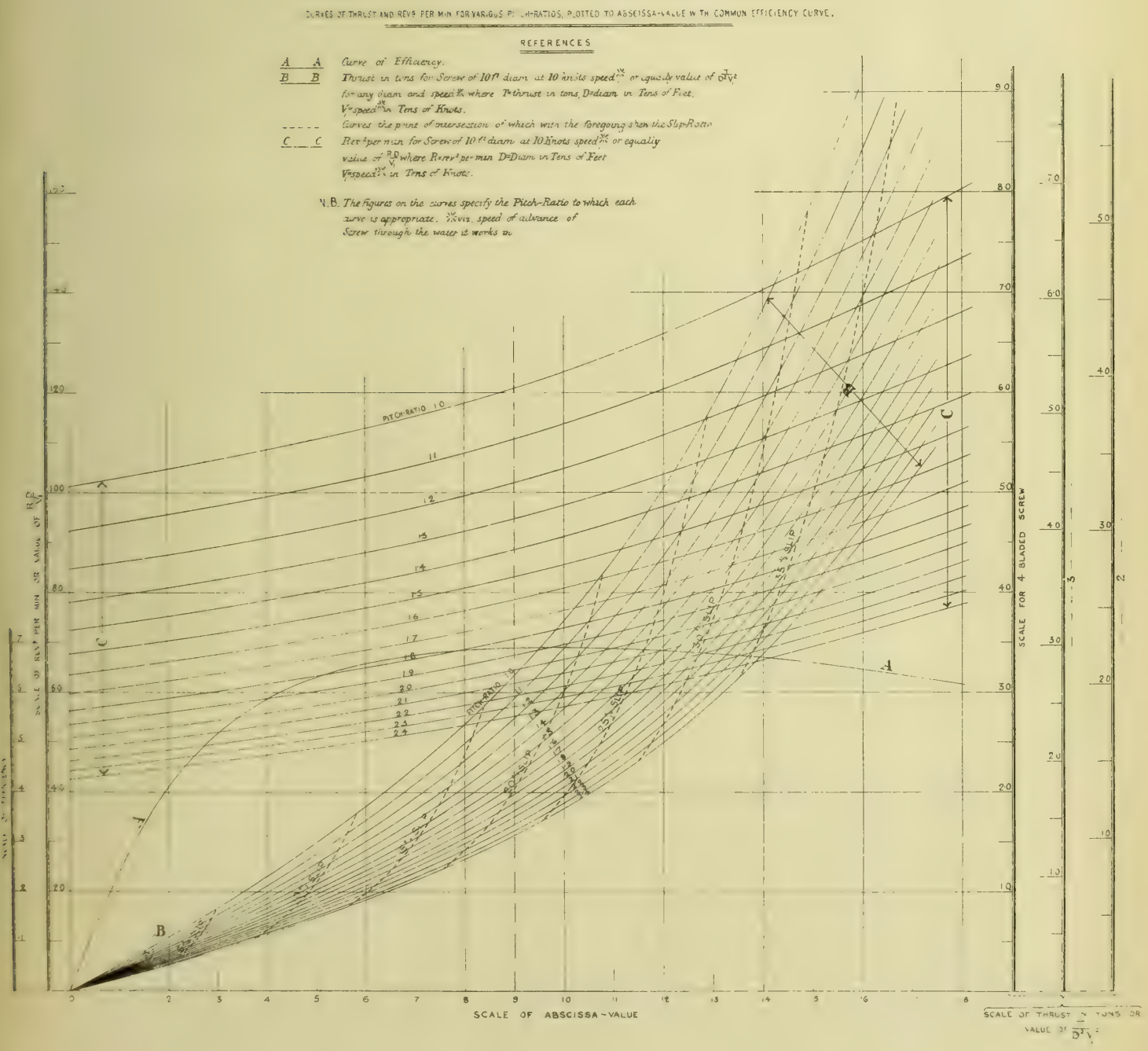


SCALE SHEWING VALUE OF MULTIPLIER "W" FOR WAKE CORRECTION

N. B VALUES FOR SINGLE-SCREW SHIPS SHEWN IN *Italics*

To Illustrate Mr. R. E. Froude's Paper on the Determination of the most suitable Dimensions for Screw Propellers.

FIG. 4.


CURVES OF THRUST AND REVS PER MIN FOR VARIOUS PITCH-RATIOS, PLOTTED TO ABSCISSA-VALUE WITH COMMON EFFICIENCY CURVE.
REFERENCES
A A Curve of Efficiency.
B B Thrust in tons for Screw of 10ft diam at 10 knots speed or equally value of T/DV²
for any diam and speed R where T=thrust in tons, D=diam in Tens of Feet,
V=speed in Tens of Knots.
Curves the point of intersection of which with the foregoing shew the Slip-Ratio
C C Revs per min for Screw of 10ft diam at 10 knots speed or equally
value of RD/V where R=revs per min D=Diam in Tens of Feet
V=speed in Tens of Knots.
N.B. The figures on the curves specify the Pitch-Ratio to which each
curve is appropriate. viz. speed of advance of
Screw through the water it works in
PITCH-RATIO 10
11
12
13
14
15
16
17
18
19
20
21
22
23
24
SCALE OF REVS PER MIN OR VALUE OF RD/V
SCALE FOR 4 BLADED SCREW
SCALE OF EFFICIENCY
SCALE OF ABSCISSA-VALUE
SCALE OF THRUST IN TONS OR VALUE OF T/D²V²

To Illustrate Mr. R. E. Froude's Paper on the Determination of the most suitable Dimensions for Screw Propellers.

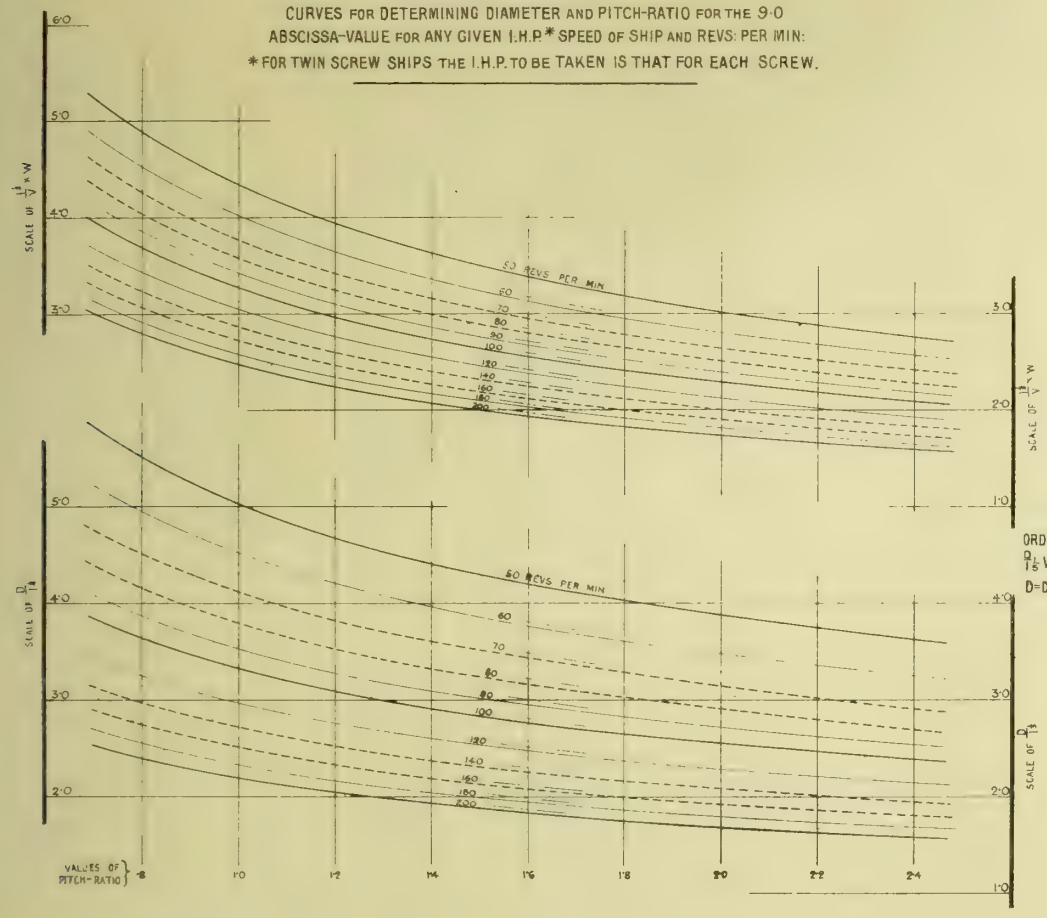
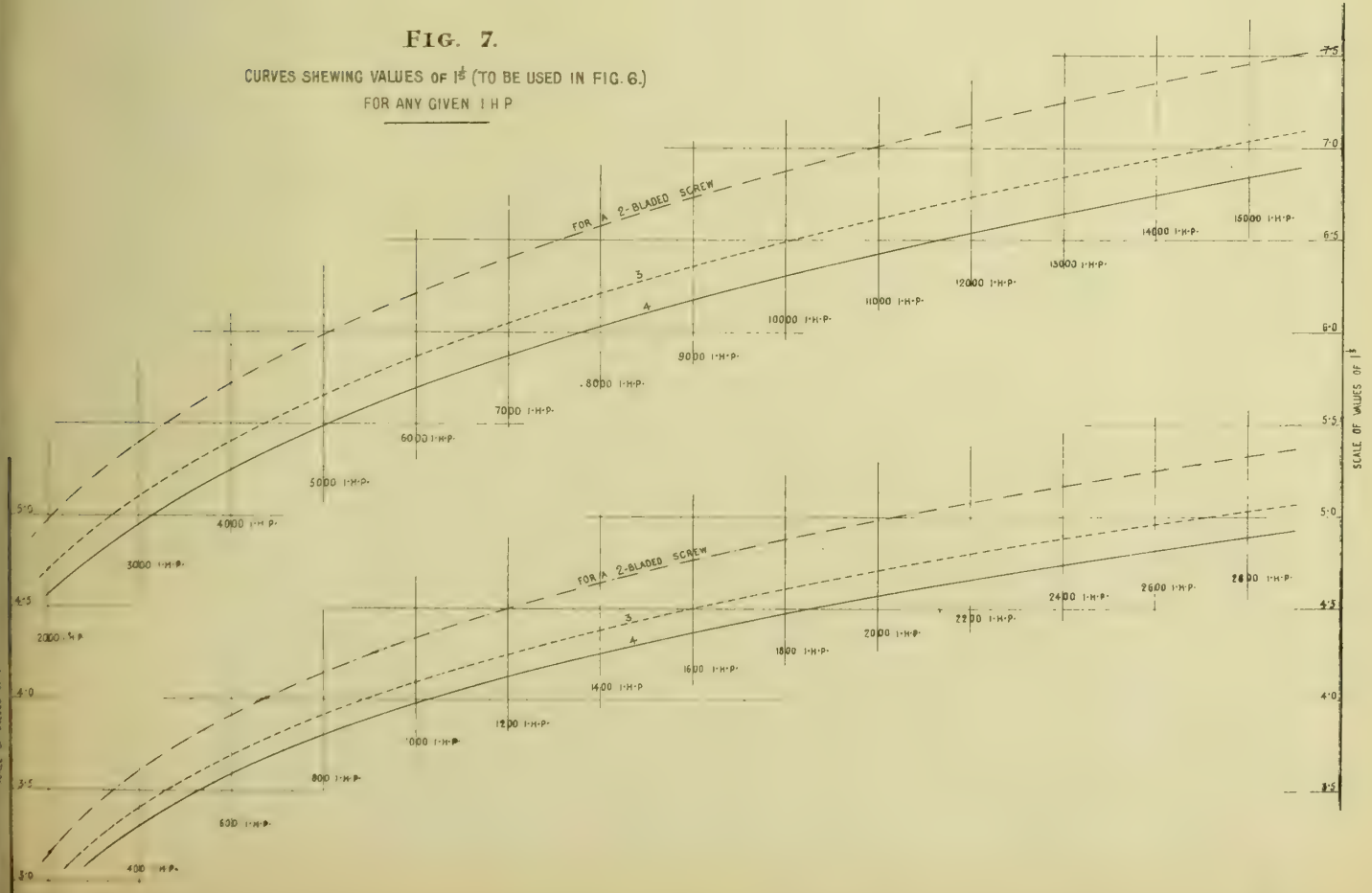


FIG. 6.

ORDINATES OF CURVES SHEW VALUES OF $\frac{V^{\frac{1}{2}}}{I^{\frac{1}{5}}} \times W$ AND $\frac{D}{I^{\frac{1}{5}}}$ WHERE V = SPEED OF SHIP IN TENS OF KNOTS
D = DIAM. IN FEET; W = MULTIPLIER FOR WAKE (SEE FIG. 8)

FIG. 7.

CURVES SHEWING VALUES OF $I^{\frac{1}{5}}$ (TO BE USED IN FIG. 6.)
FOR ANY GIVEN I.H.P.



VOLUTIONS PER MINUTE REMAINING CONSTANT.

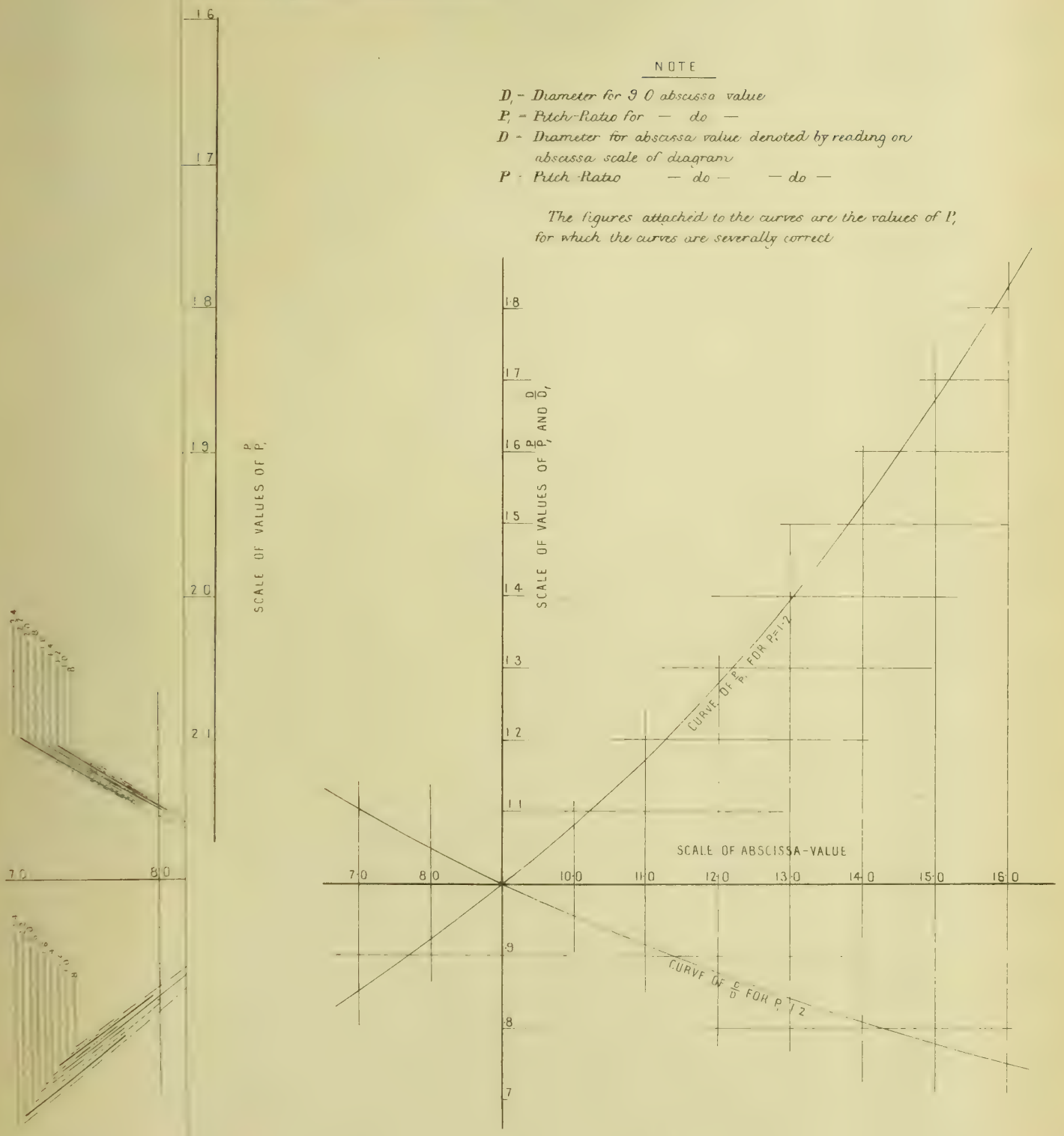

NOTE
D_1 = Diameter for 9·0 abscissa value
P_1 = Pitch-Ratio for — do —
D = Diameter for abscissa value denoted by reading on abscissa scale of diagram
P = Pitch Ratio — do — — do —
The figures attached to the curves are the values of P_1 for which the curves are severally correct
SCALE OF VALUES OF $\frac{P}{P_1}$
SCALE OF VALUES OF $\frac{P}{P_1}$ AND $\frac{D}{D_1}$
CURVE OF $\frac{P}{P_1}$ FOR P_1=1·2
CURVE OF $\frac{D}{D_1}$ FOR P_1=1·2
SCALE OF ABSCISSA-VALUE
16 17 18 19 20 21
18 17 16 15 14 13 12 11 ·9 ·8 ·7
7·0 8·0 10·0 11·0 12·0 13·0 14·0 15·0 16·0

FIG. 10.

THIS DIAGRAM MAY BE USED FOR ANY VALUE OF P_1 WHEN EXTREME ACCURACY IS NOT REQUIRED

CURVES SHEWING THE PROPORTIONATE CHANGE OF DIMENSIONS OF SCREW CORRESPONDING TO CHANGE FROM THE 9·0 ABSCISSA-VALUE TO ANY OTHER; I.H.P. AND REVOLUTIONS PER MINUTE REMAINING CONSTANT.

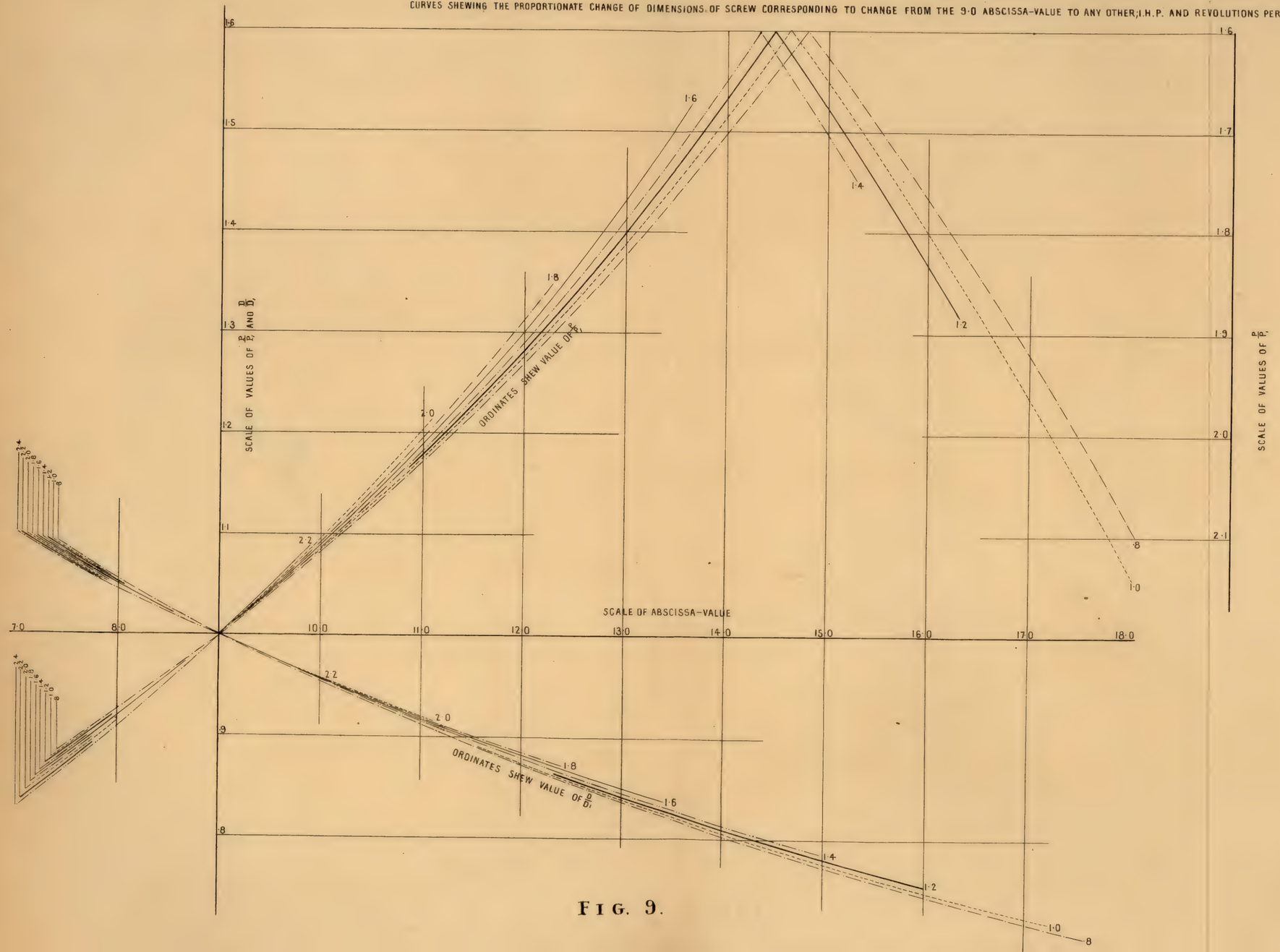

SCALE OF VALUES OF $\frac{P}{P_1}$ AND $\frac{D}{D_1}$
SCALE OF VALUES OF $\frac{P}{P_1}$
SCALE OF ABSCISSA-VALUE
ORDINATES SHEW VALUE OF $\frac{P}{P_1}$
ORDINATES SHEW VALUE OF $\frac{D}{D_1}$
7·0 8·0 10·0 11·0 12·0 13·0 14·0 15·0 16·0 17·0 18·0

FIG. 9.

NOTE.

D_1 – Diameter for 9·0 abscissa value
P_1 – Pitch-Ratio for — do —
D – Diameter for abscissa-value denoted by reading on abscissa scale of diagram
P – Pitch-Ratio — do — — do —

The figures attached to the curves are the values of P_1 for which the curves are severally correct.

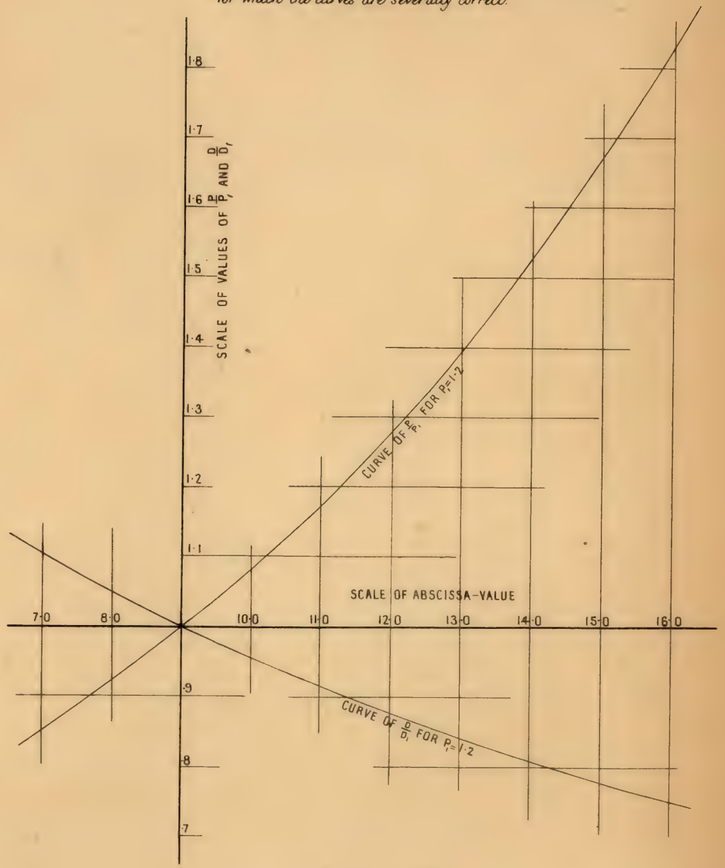
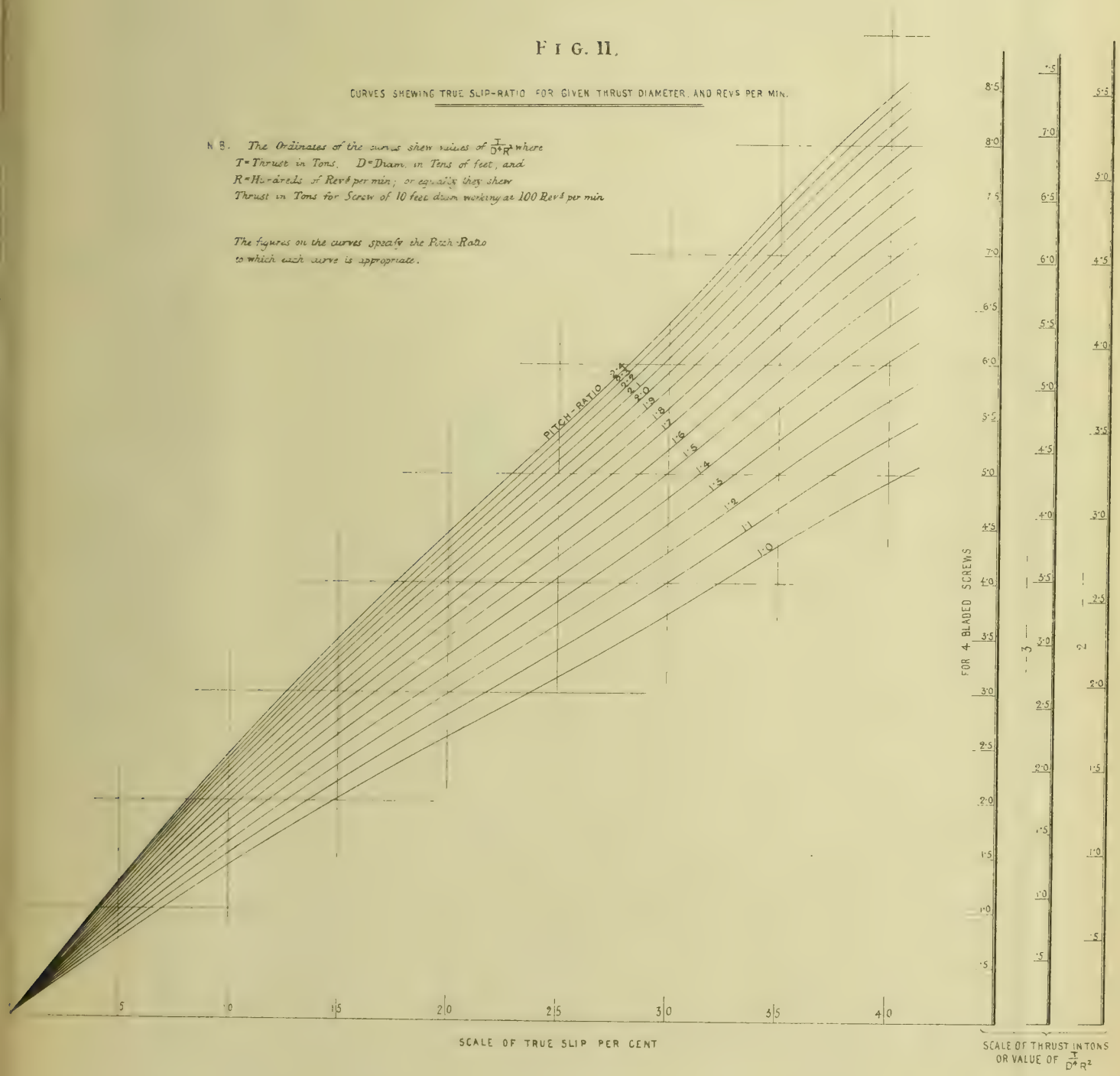

SCALE OF VALUES OF $\frac{P}{P_1}$ AND $\frac{D}{D_1}$
CURVE OF $\frac{P}{P_1}$ FOR P=1·2
CURVE OF $\frac{D}{D_1}$ FOR P=1·2
SCALE OF ABSCISSA-VALUE
7·0 8·0 10·0 11·0 12·0 13·0 14·0 15·0 16·0

FIG. 10.

THIS DIAGRAM MAY BE USED FOR ANY VALUE OF P_1 WHEN EXTREME ACCURACY IS NOT REQUIRED.

To Illustrate Mr. R. E. Froude's Paper on the Determination of the most suitable Dimensions for Screw Propellers.

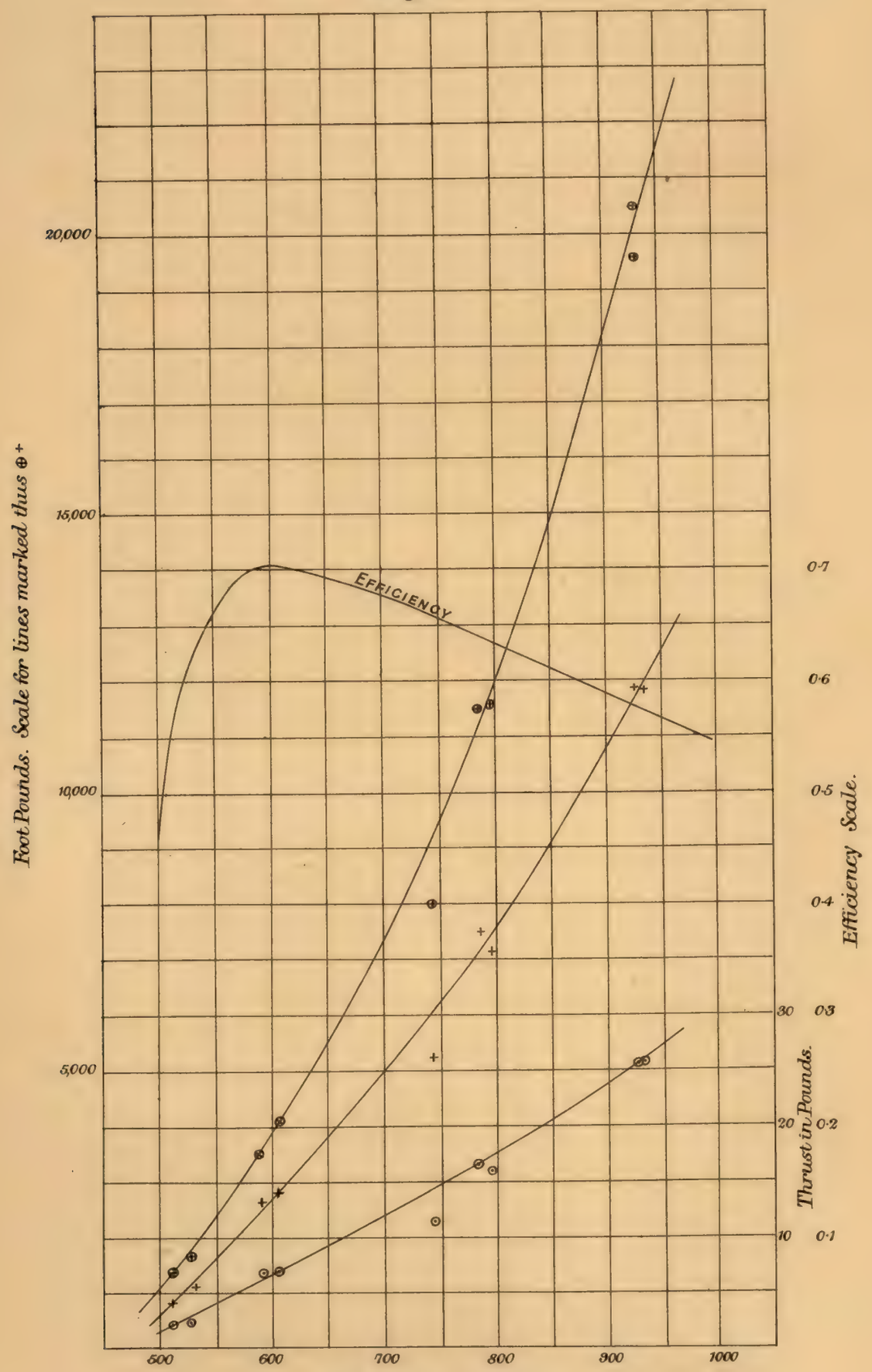
FIG. 11.



Foot Pounds. Scale for lines marked thus ⊕+

To Illustrate Mr. Sydney W. Barnaby's Remarks on Mr. R. E. Froude's Paper.

Fig. 1.

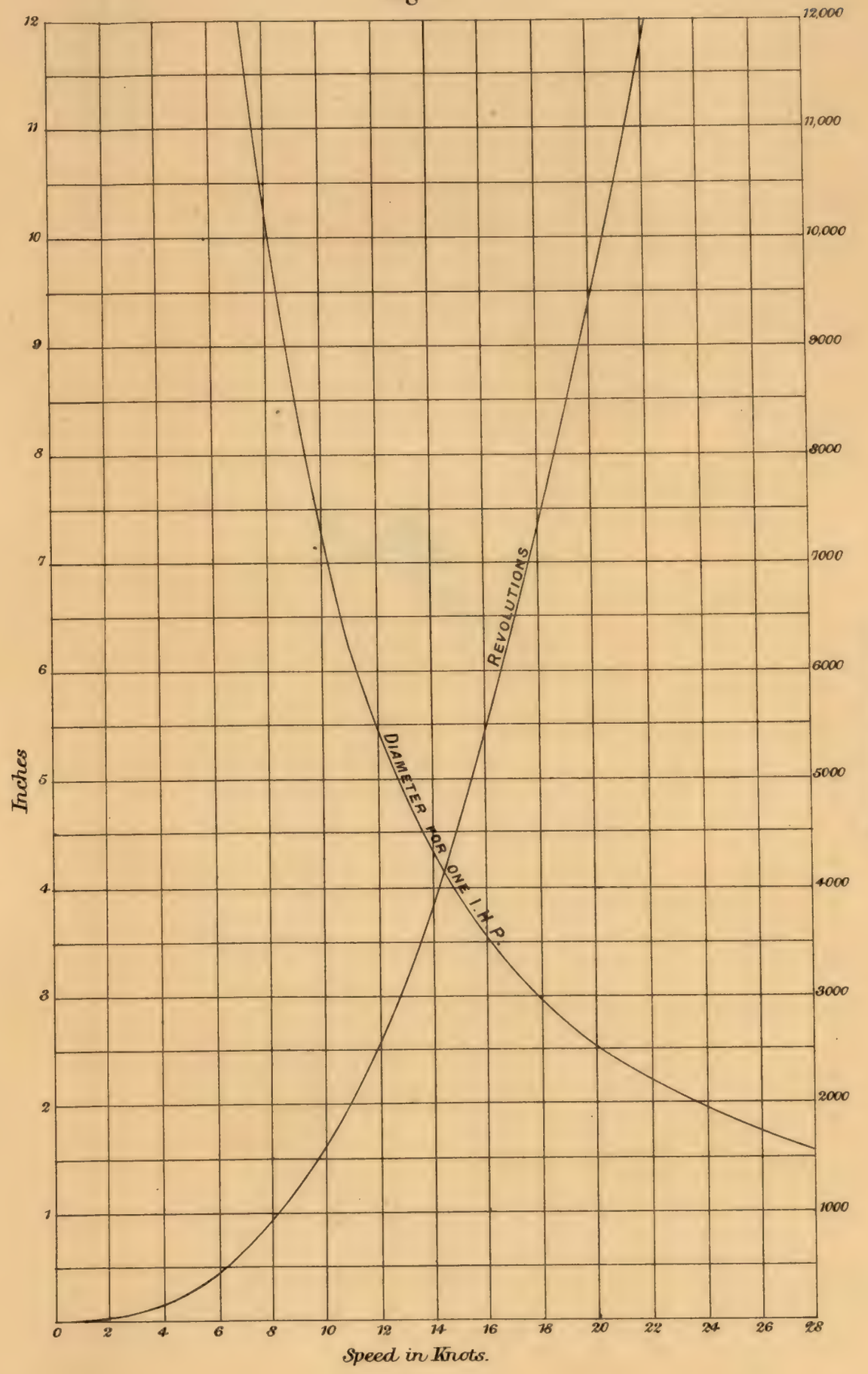


⊕ Power expended in foot-pounds. + Useful work in foot-pounds.

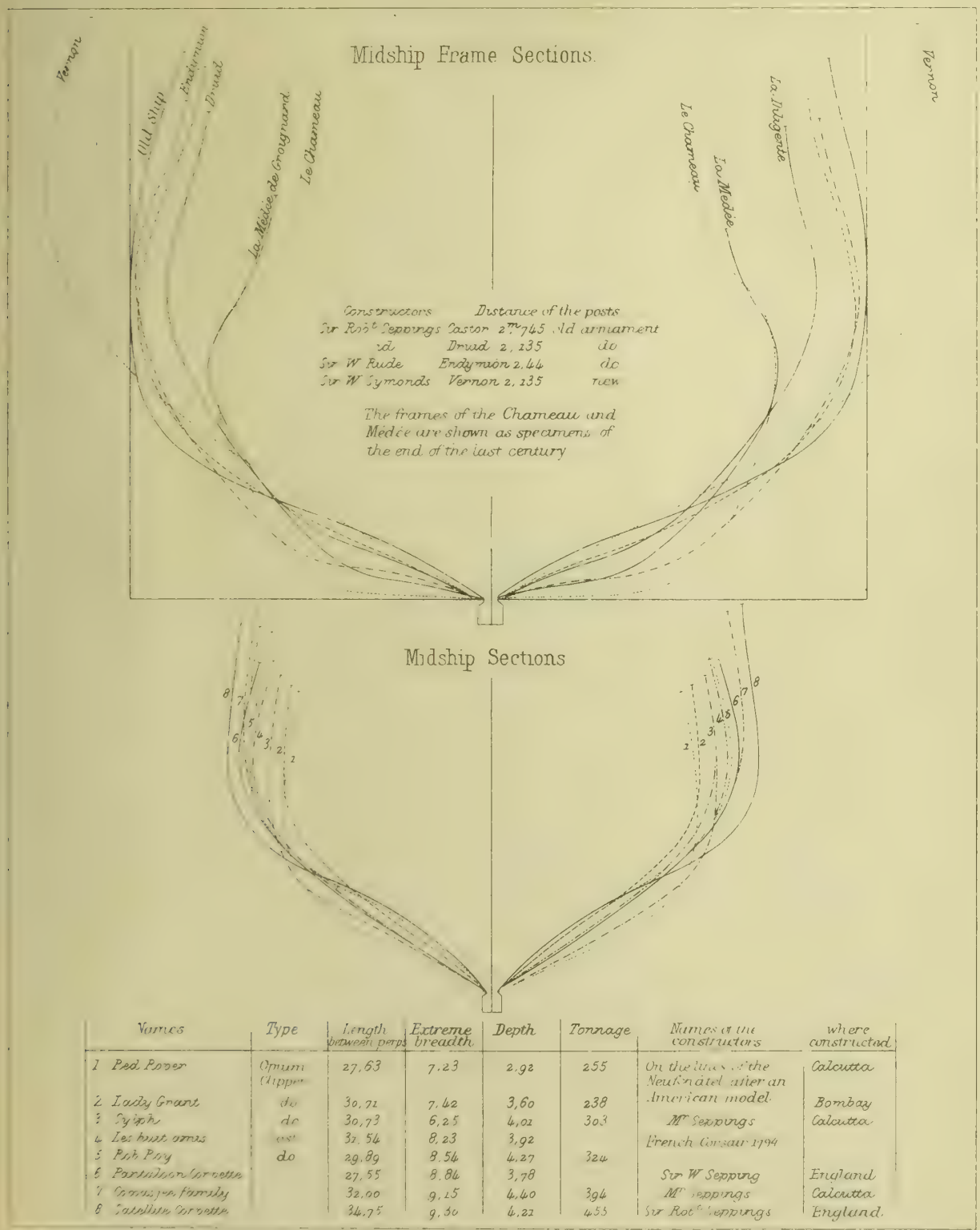
⊙ Thrust in pounds.

3 Bladed Propeller. 9 inches Diameter- 10·3 ins. Pitch.

Fig. 2.

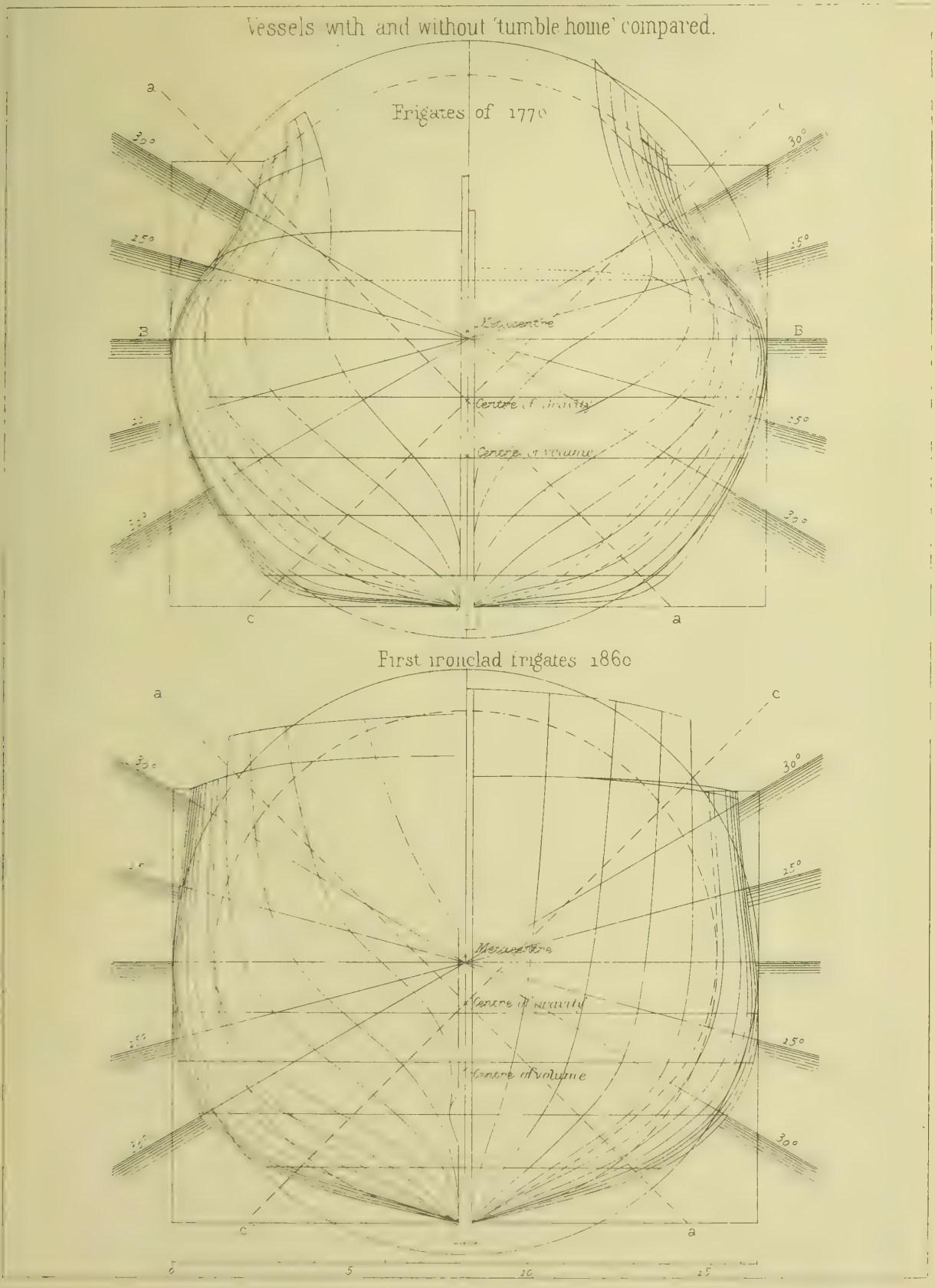


To Illustrate Vice-Admiral E. Paris' Paper, Description of an Instrument intended to Analyse the Rolling of Ships.

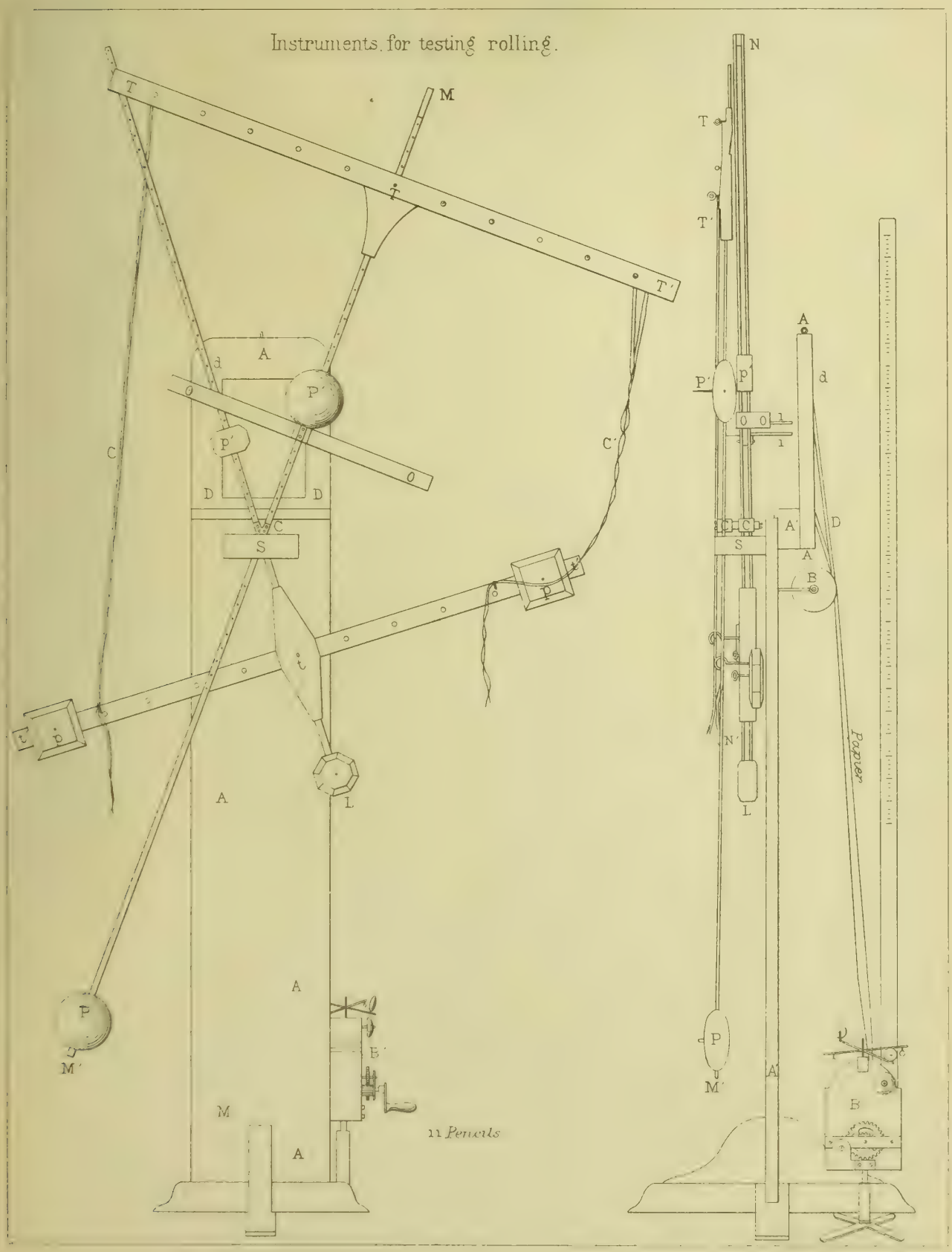


Names	Type	Length between perps	Extreme breadth	Depth	Tonnage	Names of the constructors	where constructed
1 Psd. Rover	Opium Clipper	27,63	7,23	2,92	255	On the lines of the Newfoundland after an American model	Calcutta
2 Lady Grant	do	30,72	7,42	3,60	238		Bombay
3 Sylph	do	30,73	6,25	4,02	303	Mr Seppings	Calcutta
4 Les huit amis	est	32,54	8,23	3,92		French Corsair 1794	
5 Psh Roy	do	29,89	8,54	4,27	324		
6 Partridge Corvette		27,55	8,84	3,78		Sir W Seppings	England
7 Commissioner family		32,00	9,15	4,40	394	Mr Seppings	Calcutta
8 Satellite Corvette		34,75	9,30	4,22	455	Sir Rob.t Seppings	England

To Illustrate Vice-Admiral E. Paris' Paper, Description of an Instrument intended to Analyse the Rolling of Ships.



To Illustrate Vice-Admiral E. Paris' Paper, Description of an Instrument intended to Analyse the Rolling of Ships.

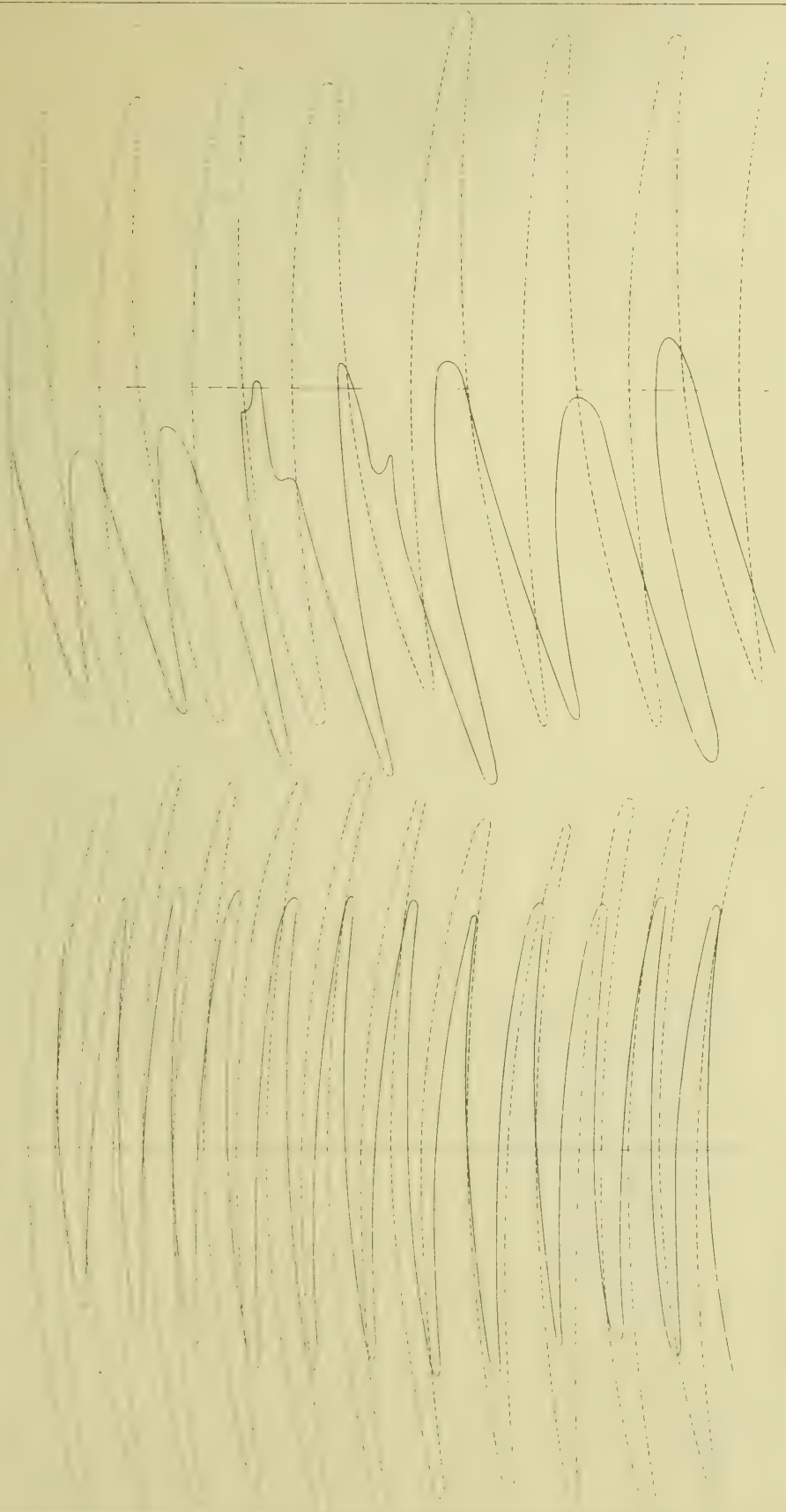


To Illustrate Vice-Admiral E. Paris' Paper, Description of an Instrument intended to Analyse the Rolling of Ships.

Simultaneous trace of the Movements of the metronome and the pendulum, expressing the motion of the Ship.

N° 2

N° 1



The full line represents the trace of the pendulum, the dotted line the action of the waves
N° 1 represents the rolling in a calm sea. N° 2 represents the curve when the ship is very much heeled over by the effect of the wind on its sails

To Illustrate Vice-Admiral E. Paris' Paper, Description of an Instrument intended to Analyse the Rolling of Ships.

Fig. 1.

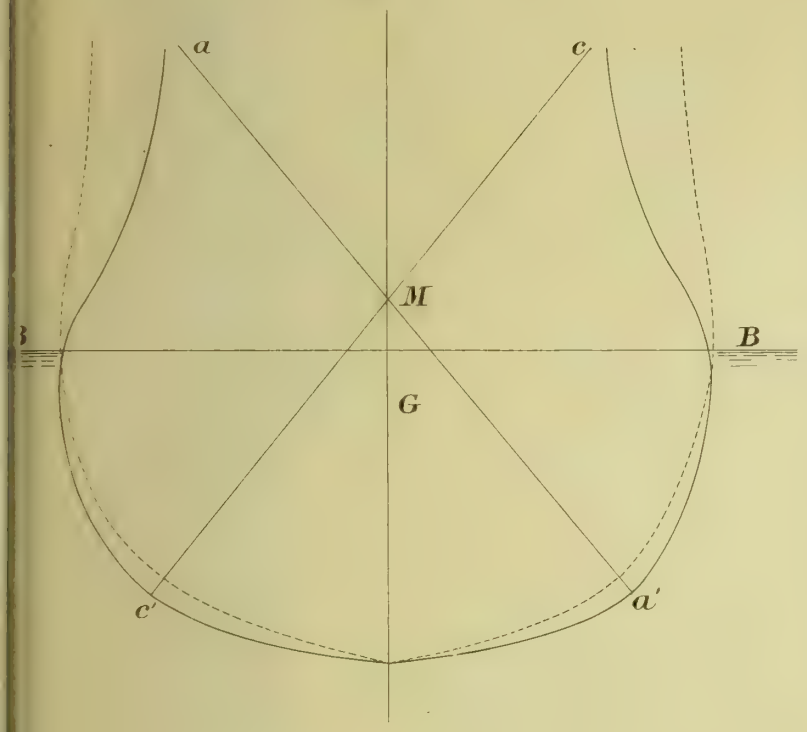
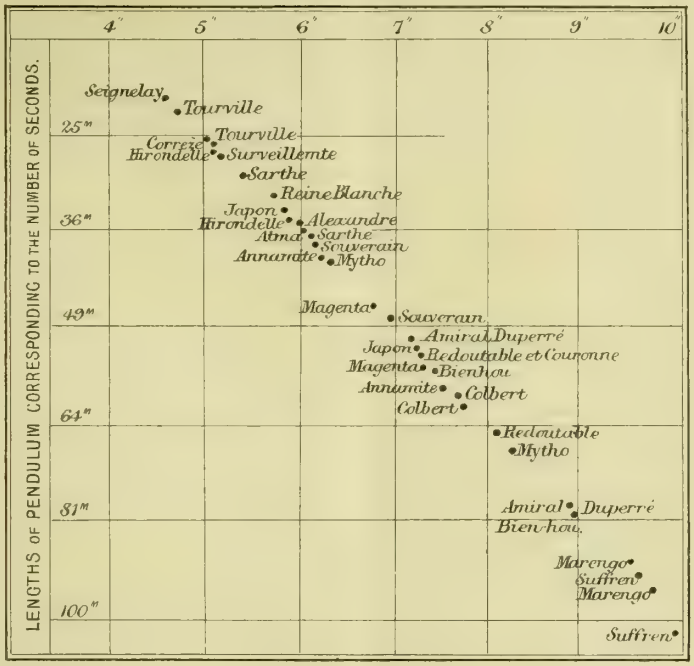


Fig. 3.



EMERSION OF A STICK OF 4 CENTIMETRES DIAMETER.

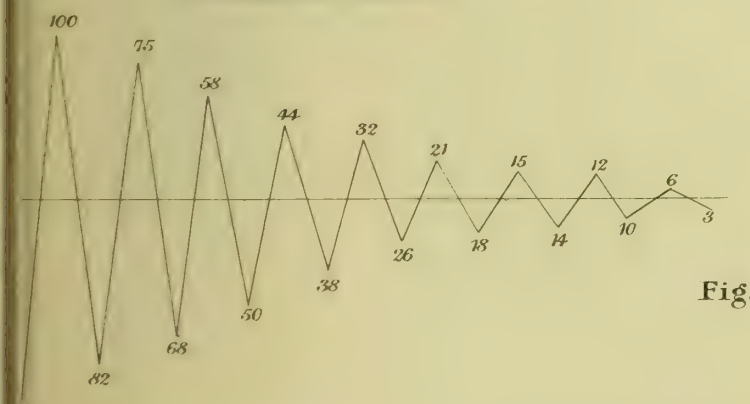
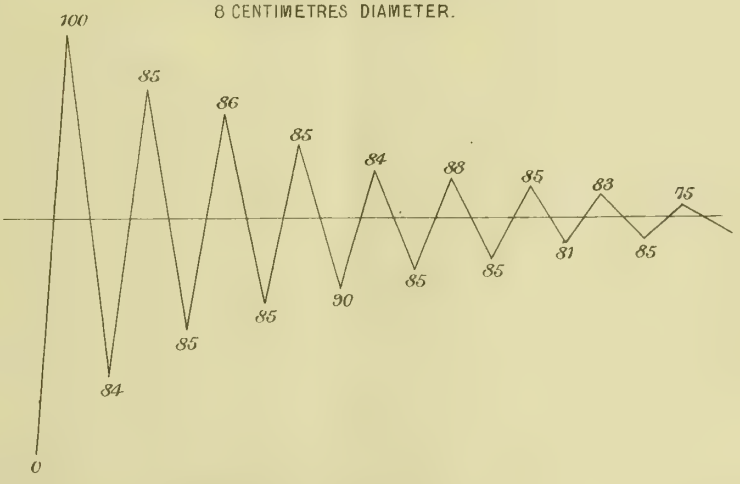


Fig. 4.

IMMERSION OF A STICK OF 8 CENTIMETRES DIAMETER.



WATER LINE OF A VESSEL WITH VERY "TUMBLE-HOME" SIDES.

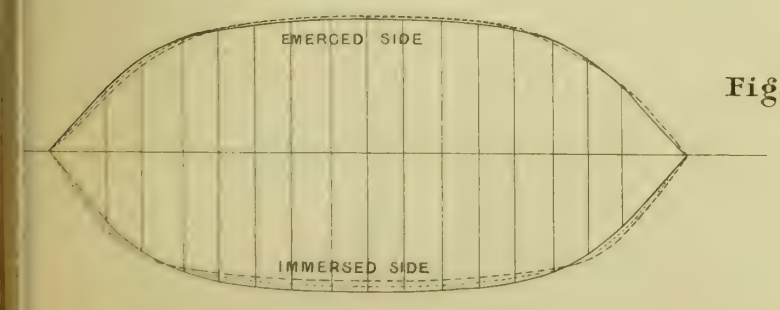
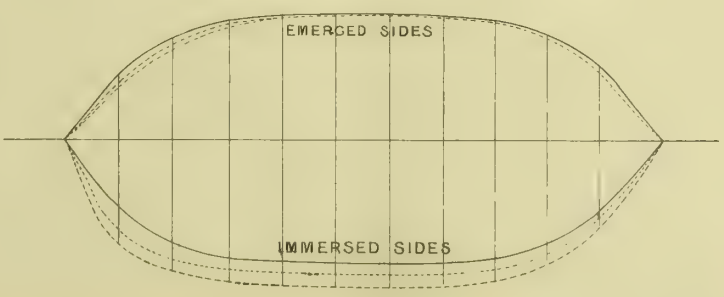


Fig. 5.

WATER LINES OF A VESSEL WITH UPRIGHT SIDES.



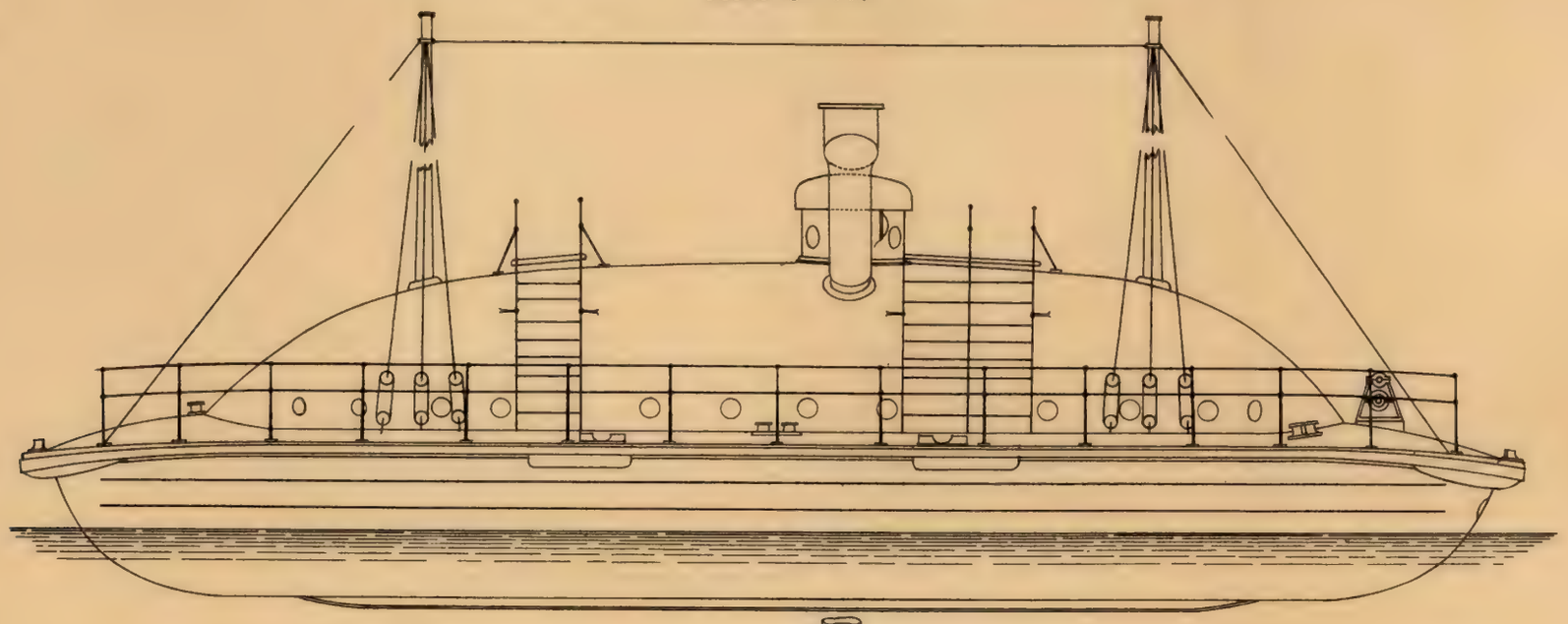
To

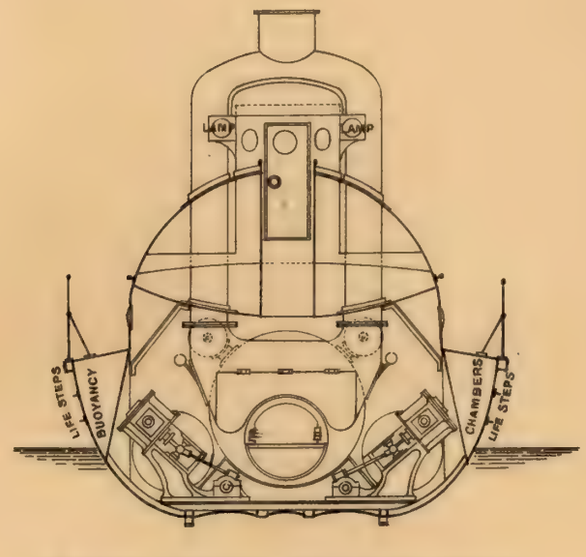
TTOM

To Illustrate Messrs. L. Benjamin and J. M. H. Taylor's Paper on a Proposed Steam Life-Boat with special Reference to its Stability.

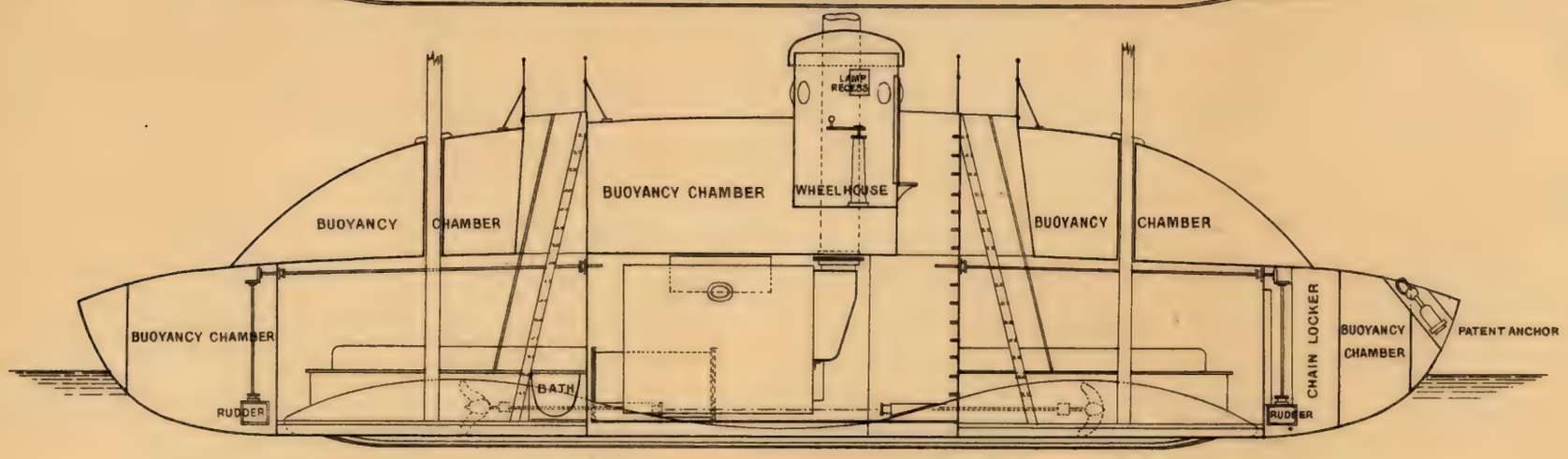
MESSRS TAYLOR & BENJAMIN'S STEAM LIFE BOAT.

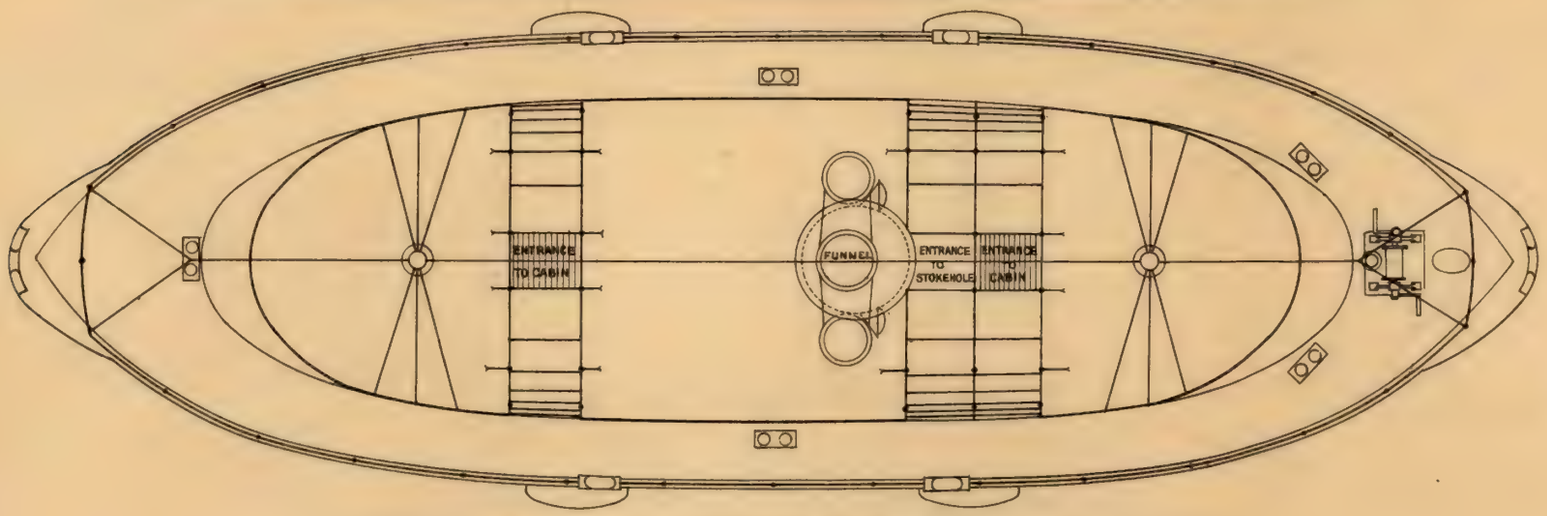
Scale ⅛" = 1 ft.

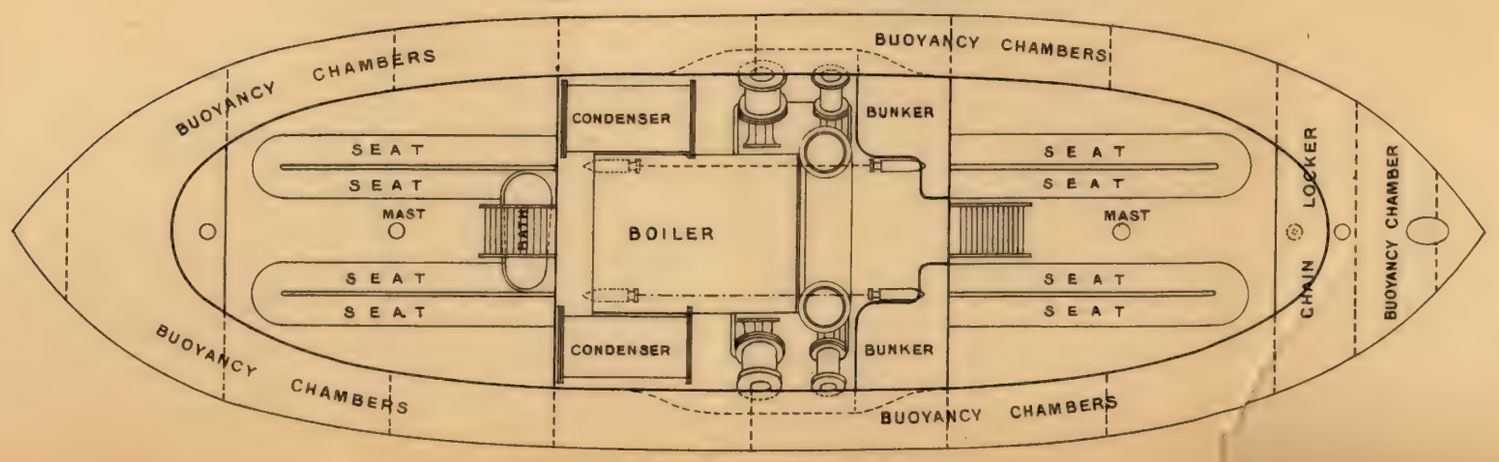


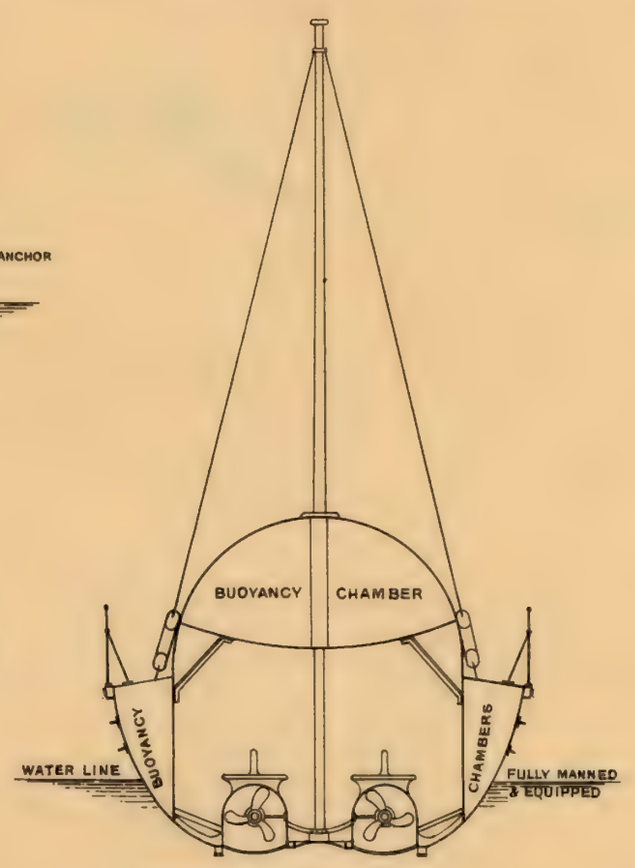


SECTION THROUGH MACHINERY SPACE.

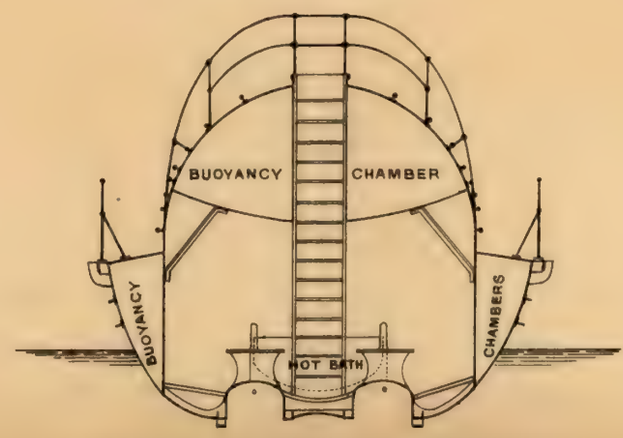








SECTION THROUGH CABIN.



SECTION THROUGH CABIN ENTRANCE.

OTTOM

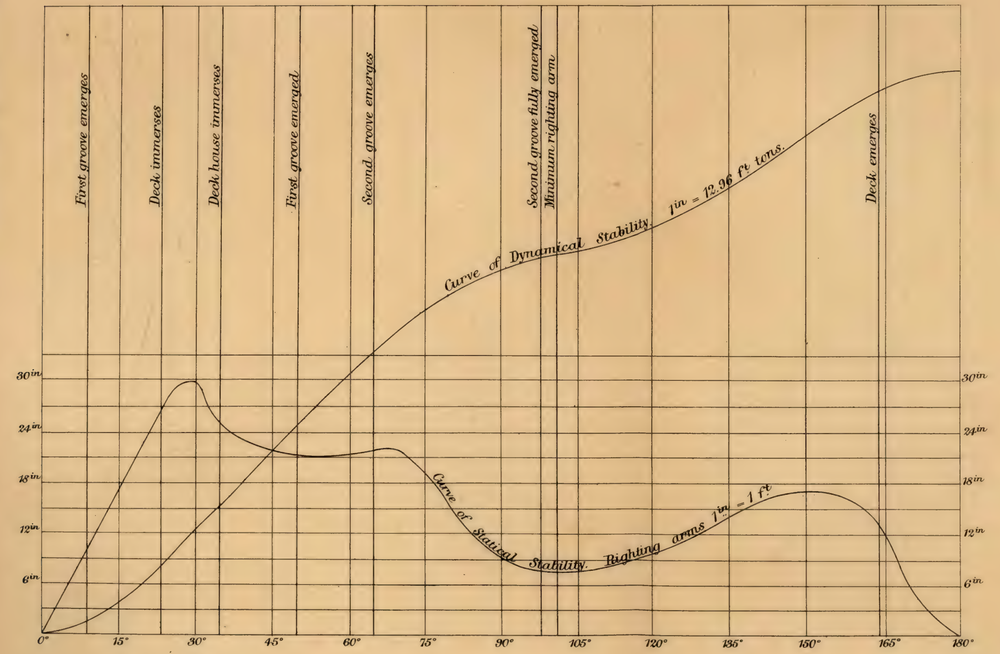
Trans. Inst. Naval Architects, Vol. XXVII, 1886.

To Illustrate Messrs. L. Benjamin and J. M. H. Taylor's Paper on a Proposed Steam Life-Boat with special Reference to its Stability

MESSRS TAYLOR & BENJAMIN'S STEAM LIFE BOAT.

STABILITY DIAGRAMS.

Fig. 1.



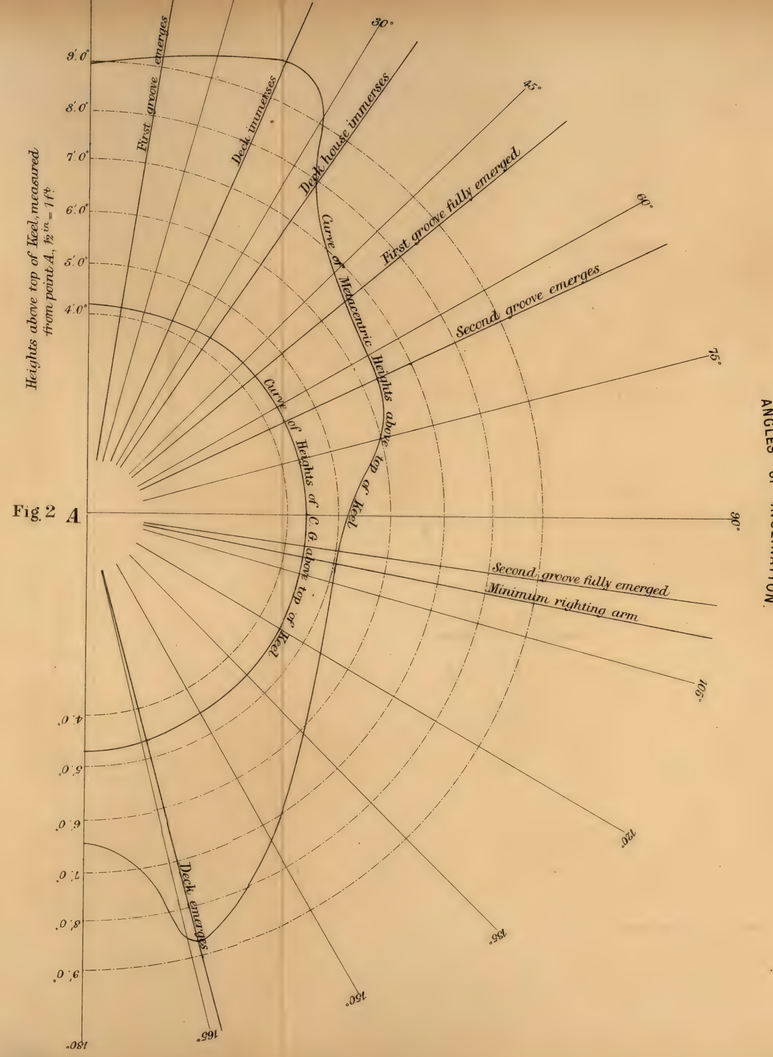


Fig. 2 *A*

ANGLES OF INCLINATION.

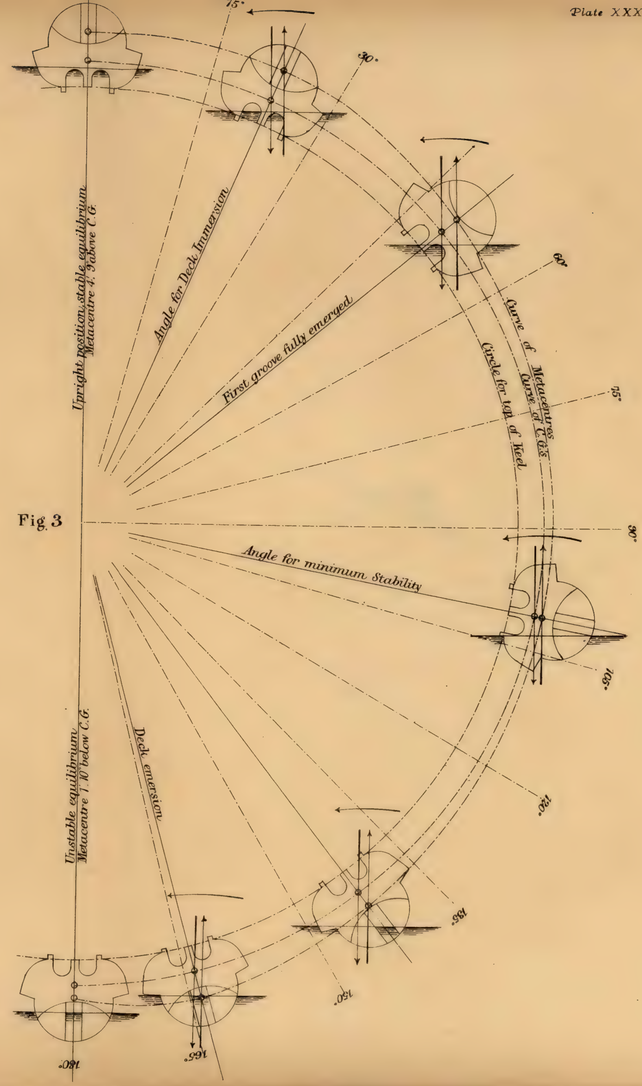
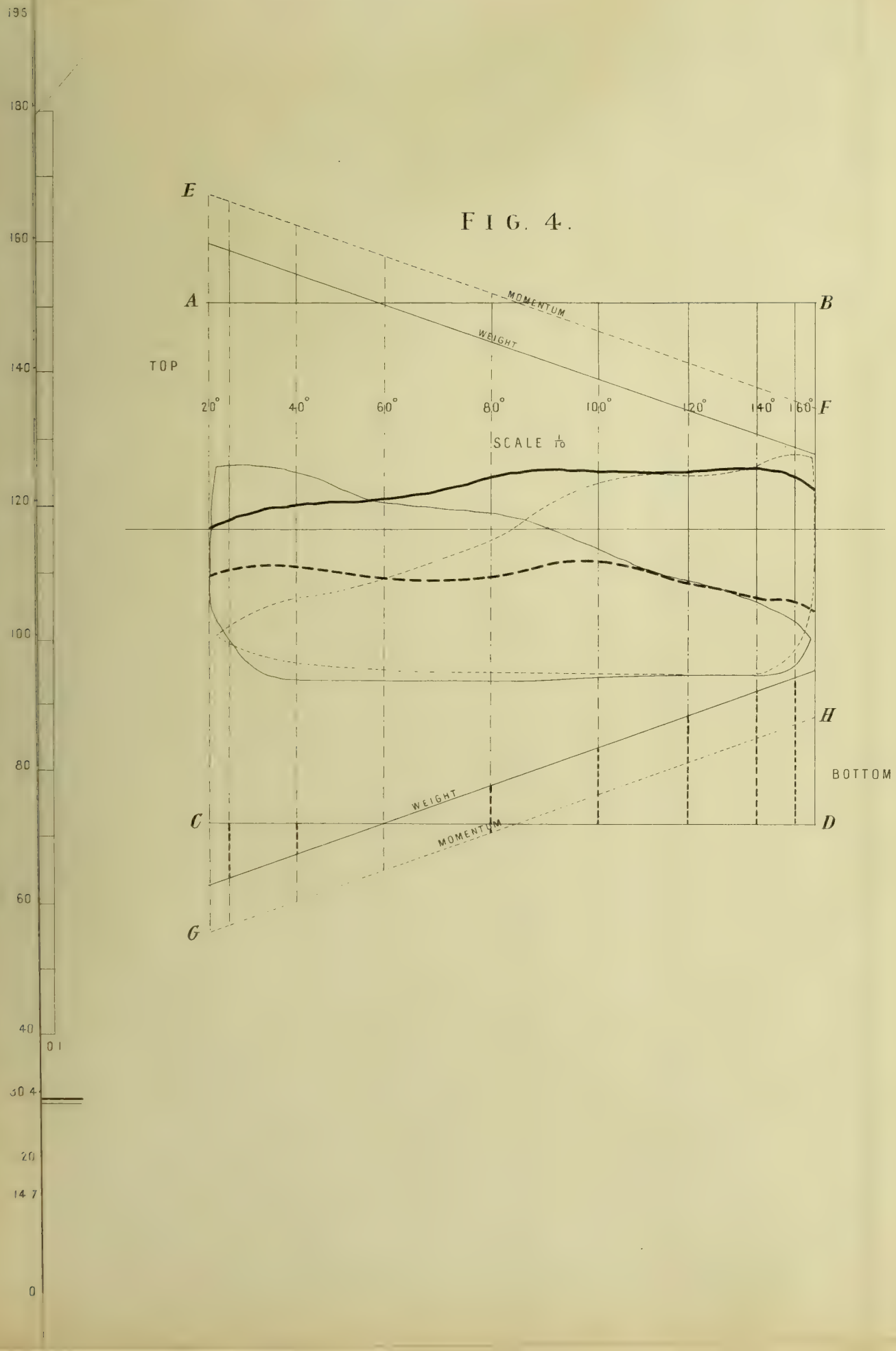


Fig. 3

Engines into Triple Expansion Engines

FIG. 4.



To Illustrate Mr. H. A. B. Cole's Paper on Converting existing Compound Engines into Triple Expansion Engines

FIG. 1.

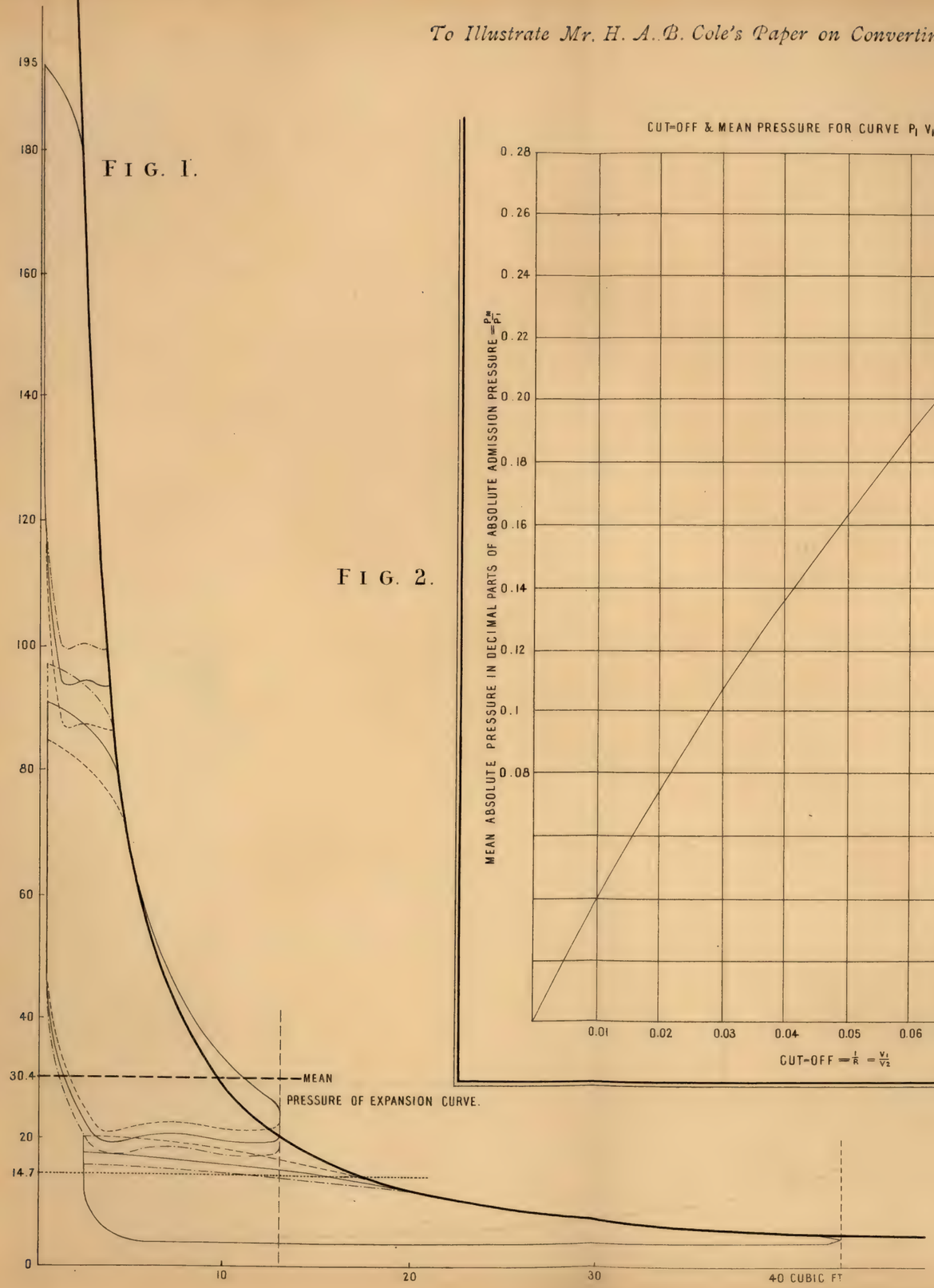


FIG. 2.

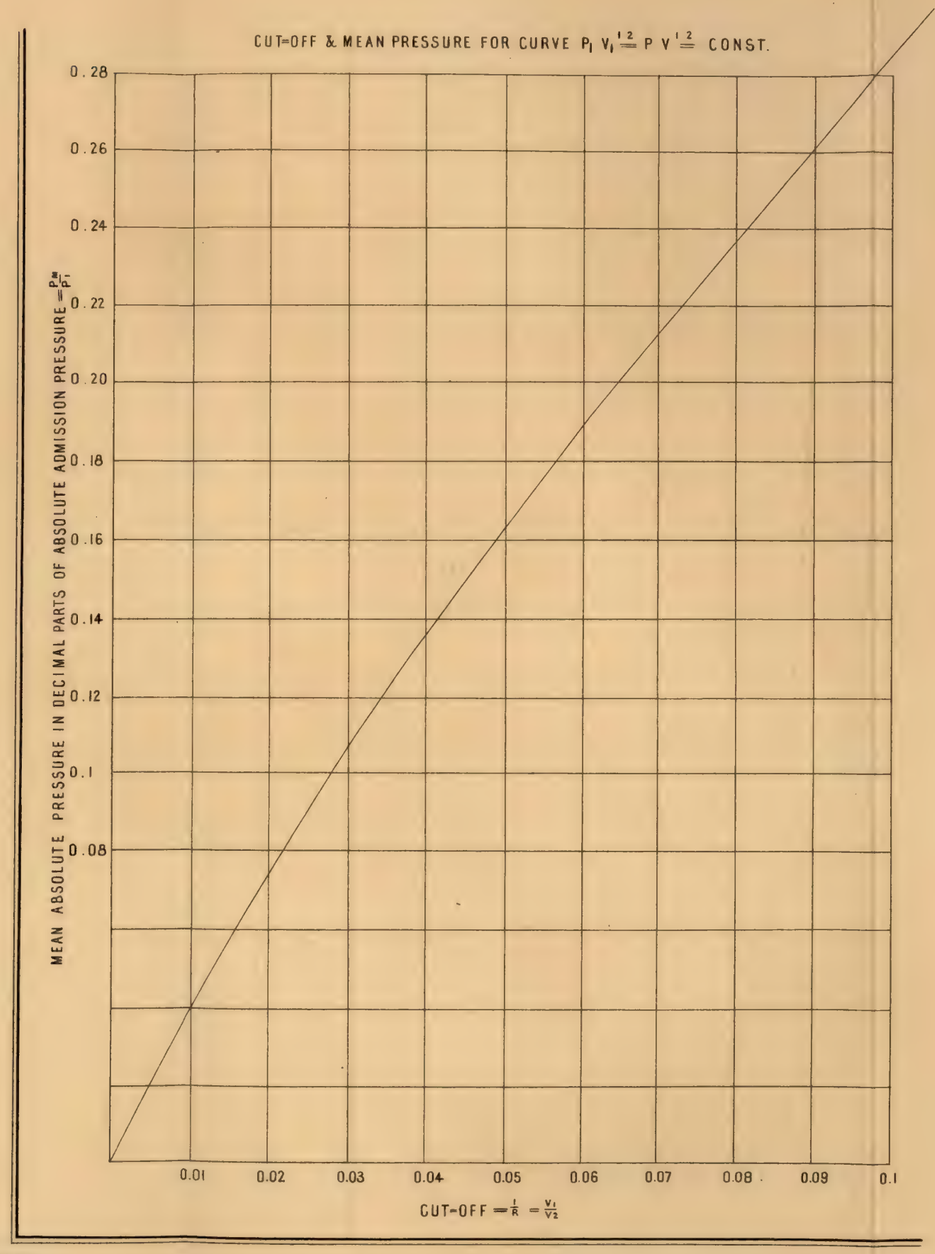
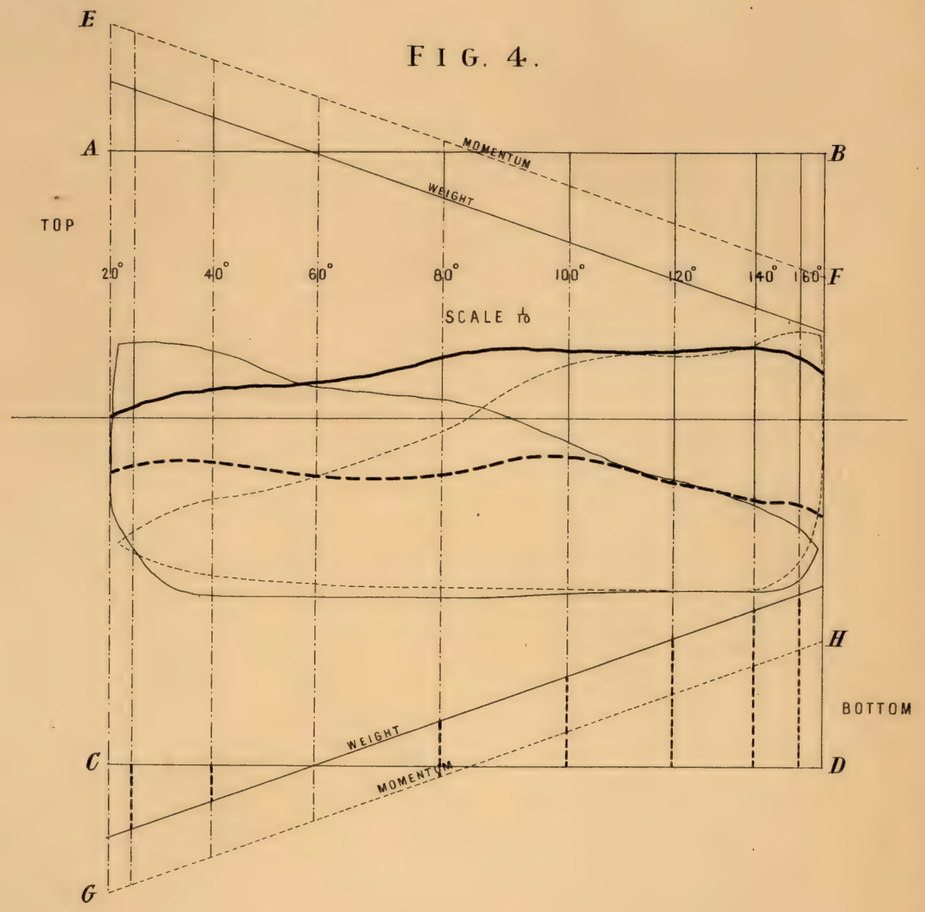


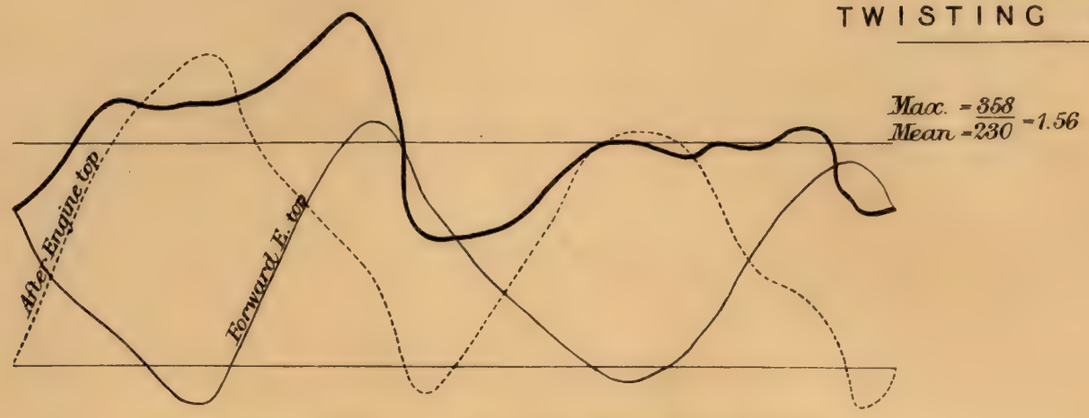
FIG. 4.



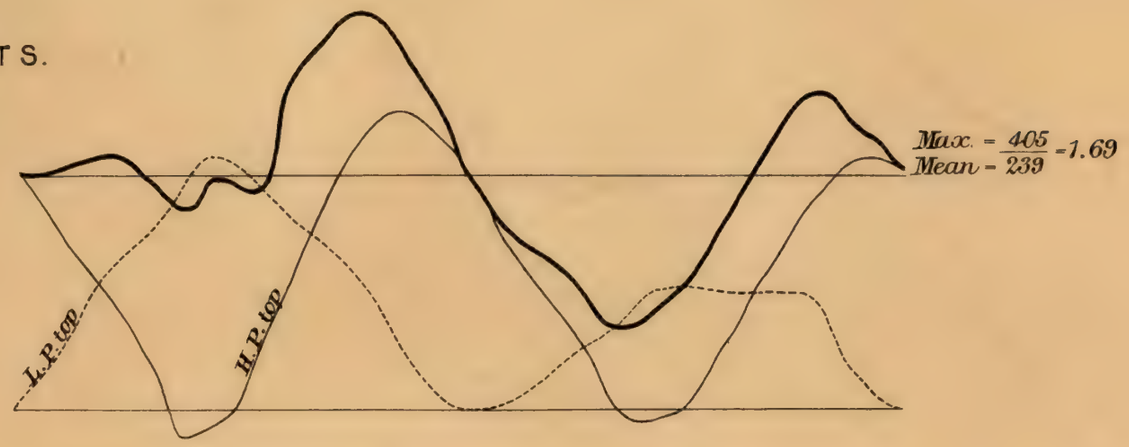
To Illustrate Mr. H. A. B. Cole's Paper on Converting existing Compound Engines into Triple Expansion Engines.

Fig. 3.

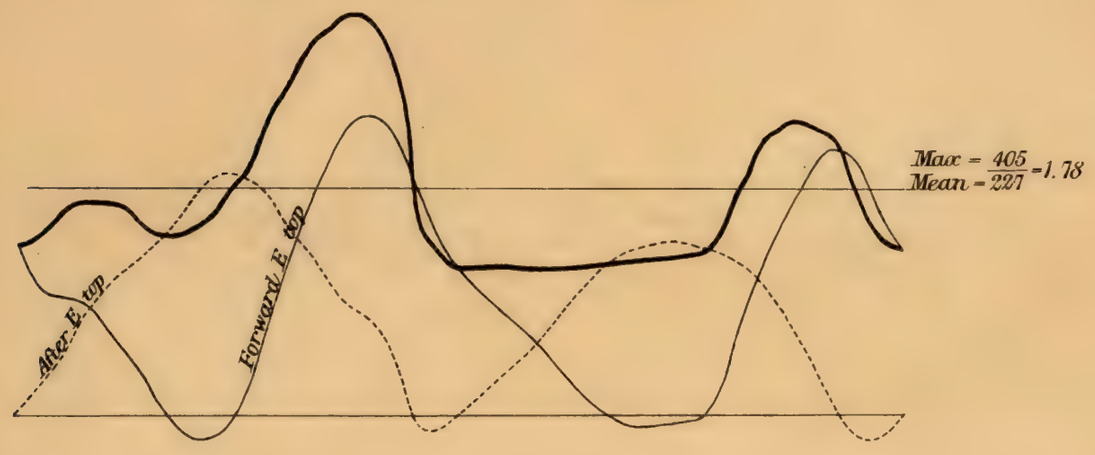
TWISTING MOMENTS.



Case I, Initial working loads on crankpins equal.

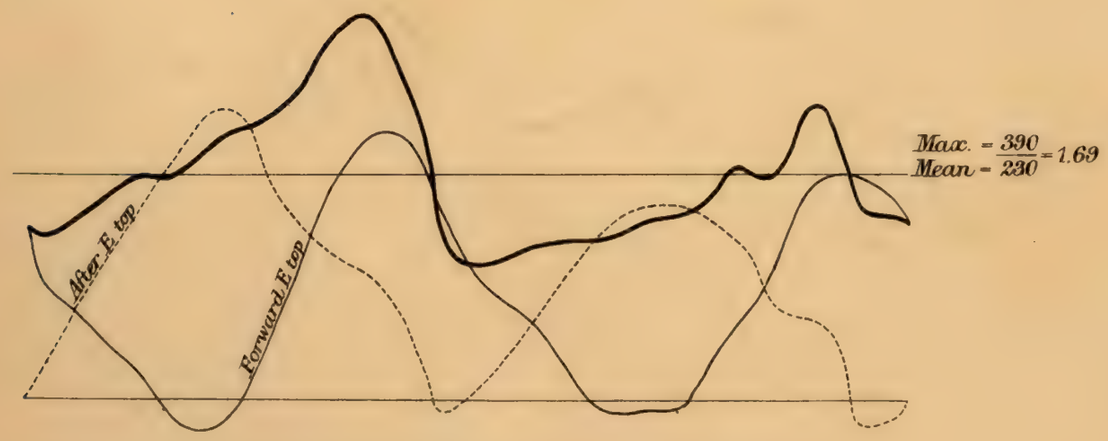


Original Engine, unaltered.

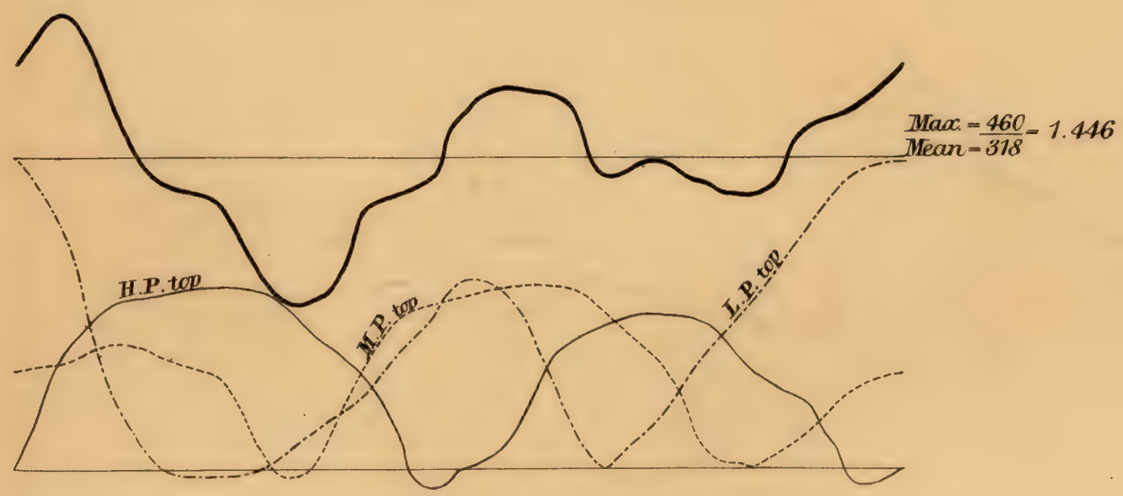


Case II. Shares of work, in both engines equal.

Three crank triple expansion Engine, 72 Revs.



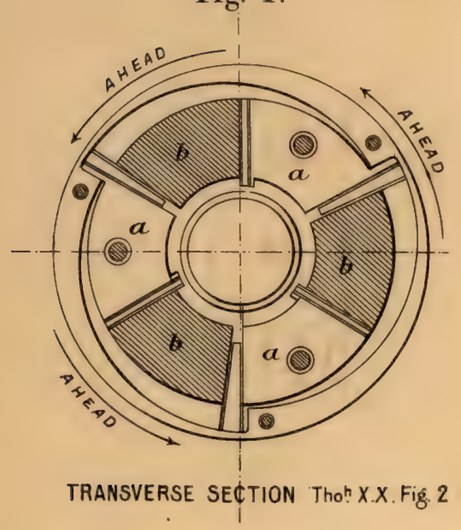
Case III, Initial absolute pressures in MP & LP cylrs. midway between those of Case I & Case II.



Three crank triple expansion Engine, 81 Revs.

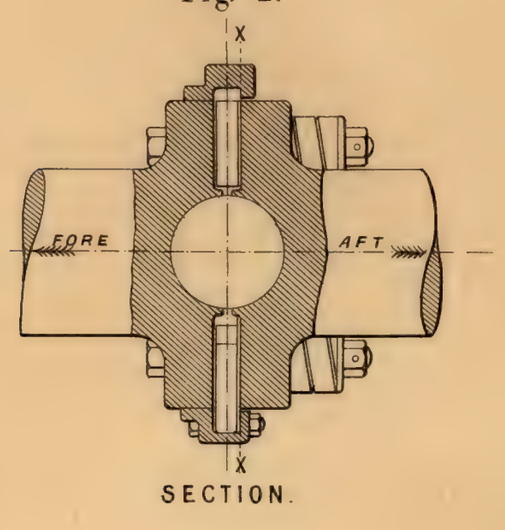
To Illustrate Mr. J. F. Hall's Paper on Flexible Crank and Propeller Shafting in Lieu of Rigid Shafting for Marine Propulsion.

Fig. 1.



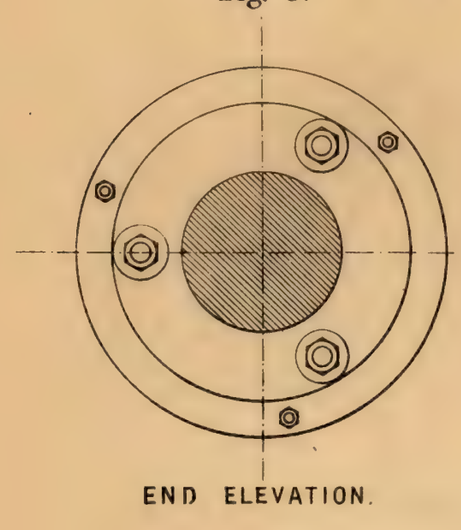
TRANSVERSE SECTION Thos X.X. Fig. 2

Fig. 2.



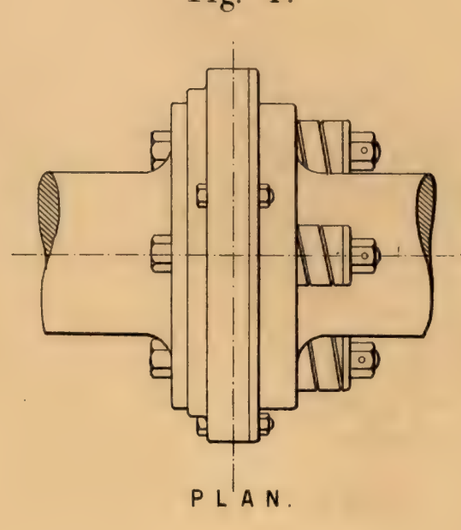
SECTION.

Fig. 3.



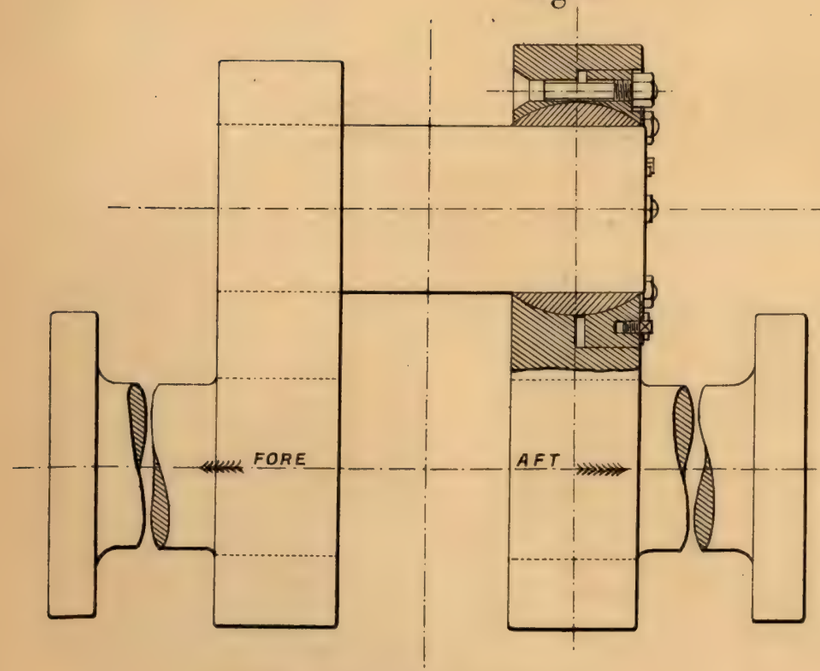
END ELEVATION.

Fig. 4.



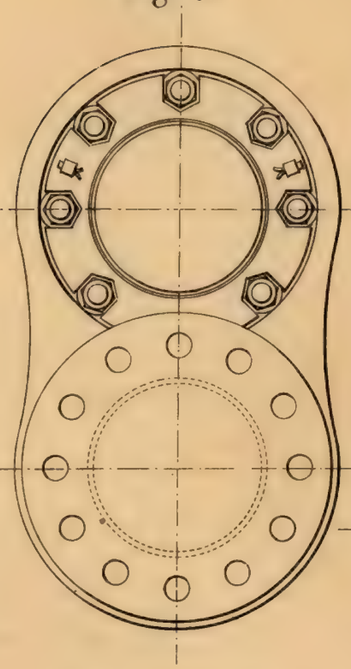
PLAN.

Fig. 5.



PART SECTIONAL PLAN OF
SECOND OR THIRD AFTWARD CRANK.

Fig. 6.



END ELEVATION.

Fig. 7.

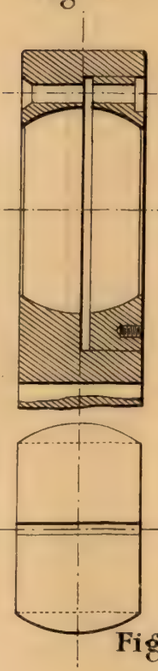
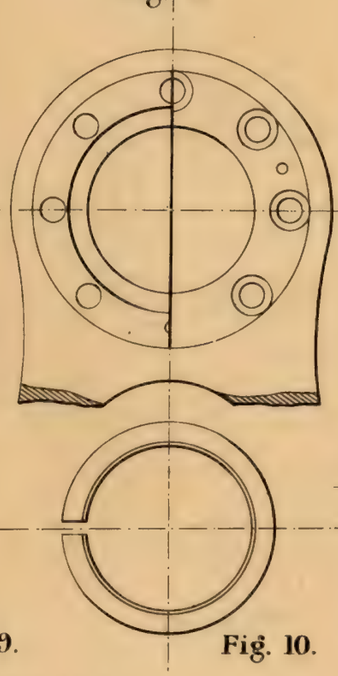


Fig. 8.



DETAILS.

Fig. 9.

Fig. 10.

Fig. 11.

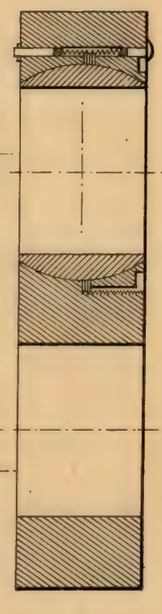


Fig. 12.

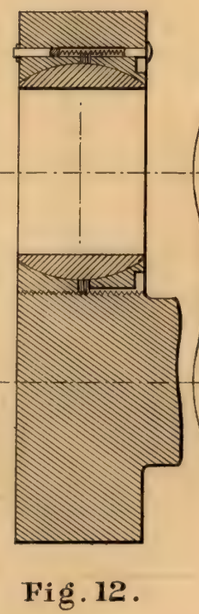
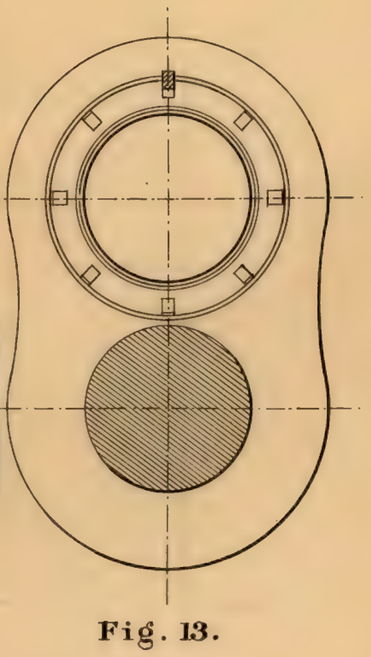


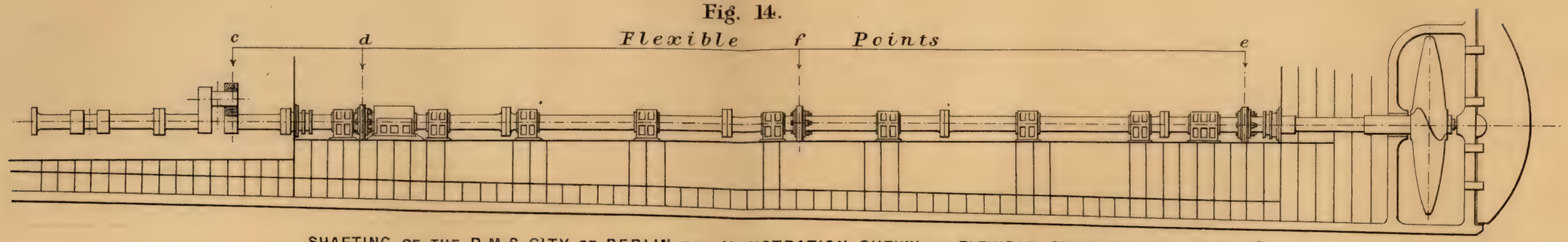
Fig. 13.



Scale 1/24.

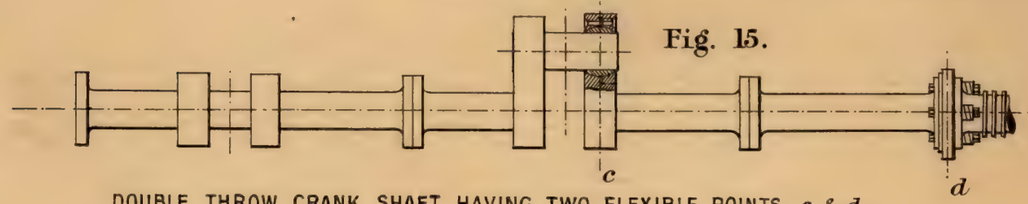
To Illustrate Mr. J. F. Hall's Paper on Flexible Crank and Propeller Shafting in Lieu of Rigid Shafting for Marine Propulsion.

Fig. 14.



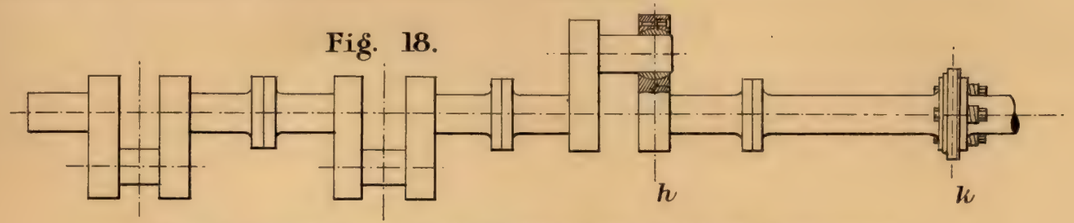
SHAFTING OF THE R.M.S. CITY OF BERLIN FOR ILLUSTRATION SHEWN AS FLEXIBLE SHAFTING. *Scale 1/192nd*

Fig. 15.



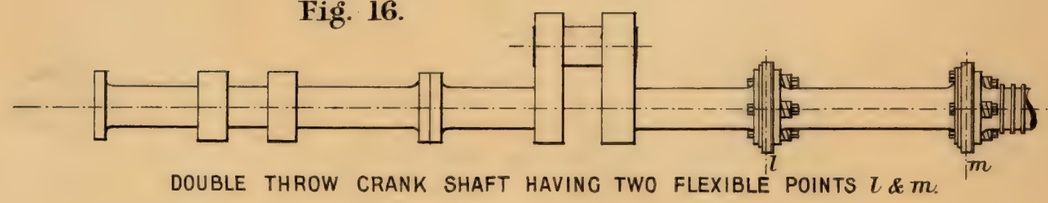
DOUBLE THROW CRANK SHAFT HAVING TWO FLEXIBLE POINTS *c & d.*

Fig. 18.



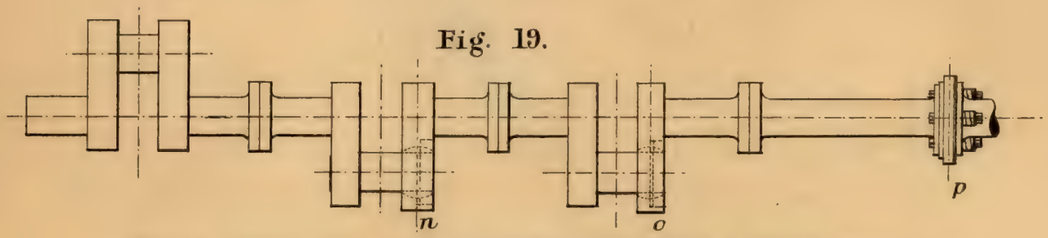
TRIPLE THROW CRANK SHAFT HAVING TWO FLEXIBLE POINTS *h & k.*

Fig. 16.



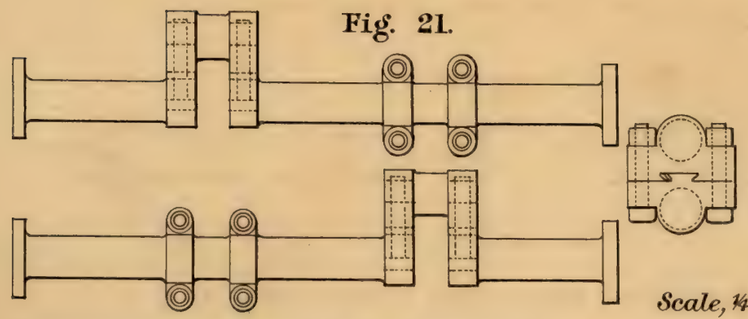
DOUBLE THROW CRANK SHAFT HAVING TWO FLEXIBLE POINTS *l & m.*

Fig. 19.



TRIPLE THROW CRANK SHAFT HAVING THREE FLEXIBLE POINTS *n, o & p.*

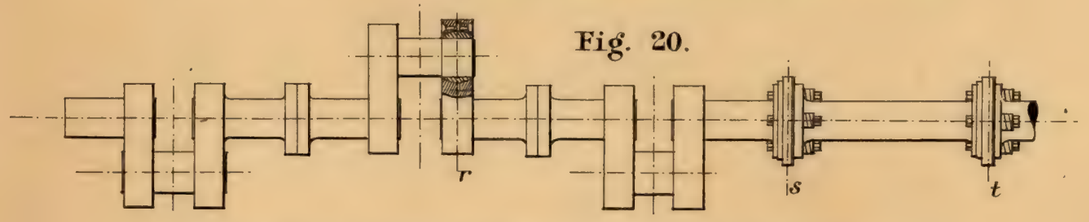
Fig. 21.



Scale, ¼"-1ft

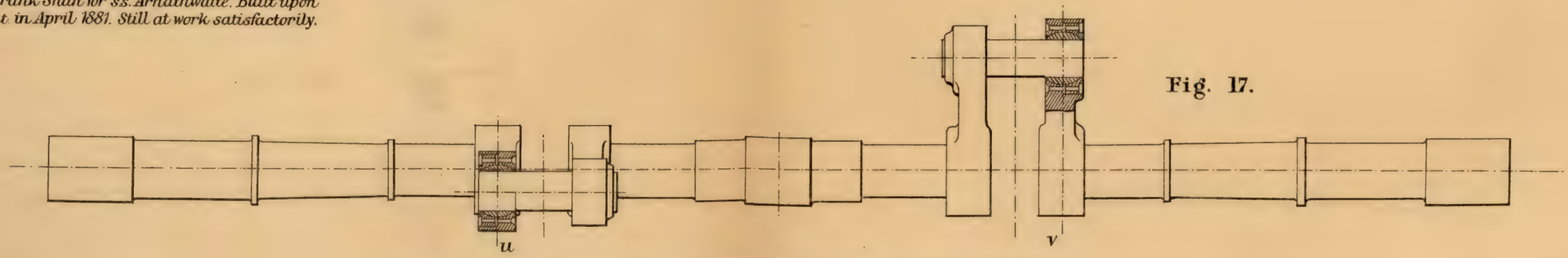
Double Throw Crank Shaft for ss. "Arnathwaite". Built upon Turton's patent in April 1881. Still at work satisfactorily.

Fig. 20.



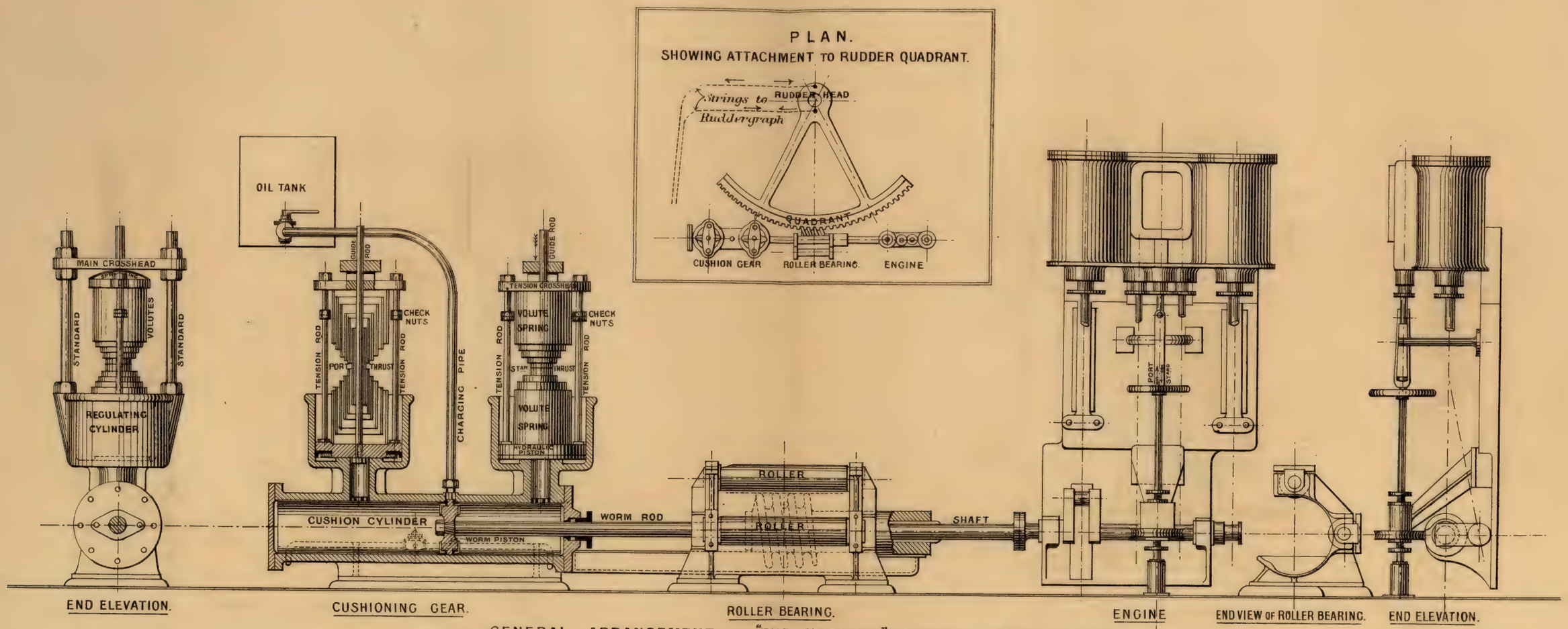
TRIPLE THROW CRANK SHAFT HAVING THREE FLEXIBLE POINTS *r, s & t*

Fig. 17.



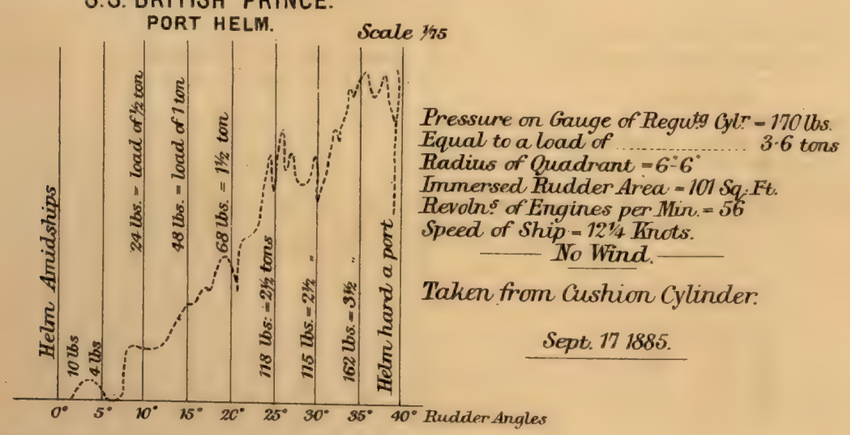
DOUBLE THROW PADDLE CRANK SHAFT HAVING TWO FLEXIBLE POINTS *u & v.*

To Illustrate Mr. Arthur J. Maginnis' Paper on a New System of Steering Gear and Rudder Strains recorded by it.



GENERAL ARRANGEMENT OF "THE MAGINNIS" CUSHION STEERING GEAR.

DIAGRAM OF THRUST OR LOAD ON QUADRANT
S.S. "BRITISH PRINCE."


PORT HELM.
Scale 1/15
Helm Amidships
10 lbs
4 lbs
24 lbs. = load of ½ ton
48 lbs. = load of 1 ton
68 lbs. = 1½ ton
118 lbs. = 2½ tons
115 lbs. = 2½ "
162 lbs. = 3½ "
Helm hard a port.
0° 5° 10° 15° 20° 25° 30° 35° 40° Rudder Angles

Pressure on Gauge of Regulg Cylr = 170 lbs.
Equal to a load of ..... 3·6 tons
Radius of Quadrant = 6'·6"
Immersed Rudder Area = 101 Sq. Ft.
Revolns of Engines per Min. = 56
Speed of Ship = 12¼ Knots.
No Wind.

Taken from Cushion Cylinder.

Sept. 17 1885.

First Diagram ever taken of Rudder Strains.

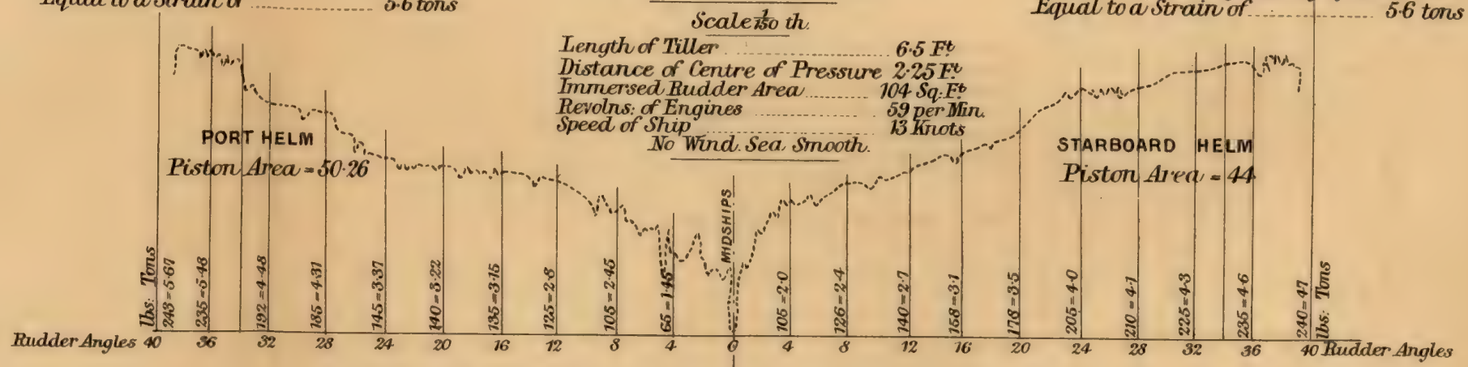
DIAGRAMS OF RUDDER STRAINS.
S.S. "BRITISH PRINCE"

Scale 1/30 th.

Length of Tiller ..... 6·5 Ft
Distance of Centre of Pressure 2·25 Ft
Immersed Rudder Area ..... 104 Sq. Ft
Revolns. of Engines ..... 59 per Min.
Speed of Ship ..... 13 Knots
No Wind. Sea Smooth.

Press. Gauge of Regulating Cylr = 250 lbs.
Equal to a Strain of ..... 5·6 tons

Press. Gauge of Regulating Cylr = 290 lbs.
Equal to a Strain of ..... 5·6 tons



Taken by Richards' Indicator from Cushion Cylinder. Dec. 10 1885.

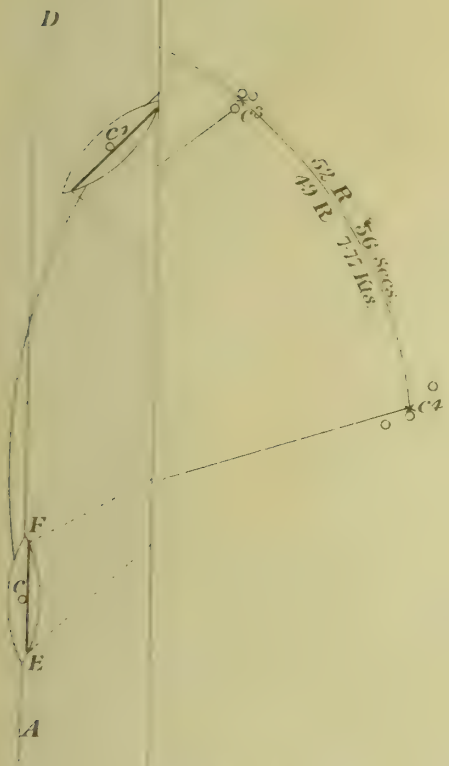


Fig. 5.

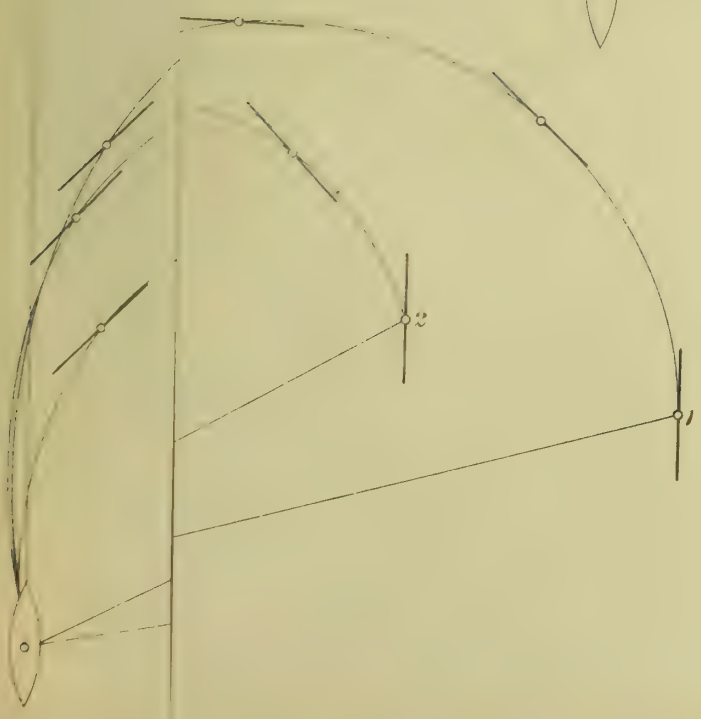
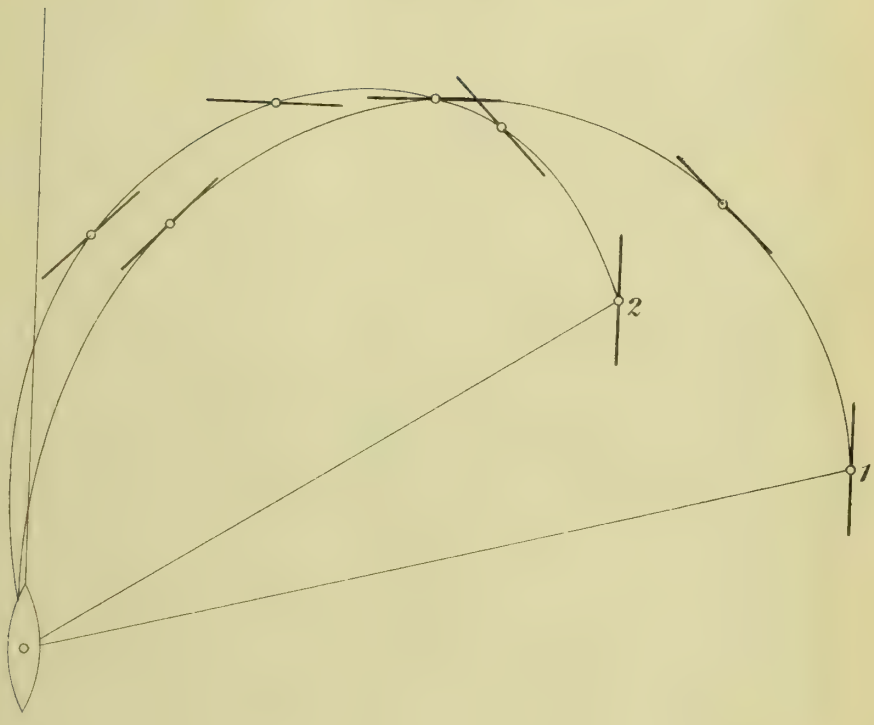


Fig. 4.



To Illustrate Captain P. H. Colomb's Paper on Recent Measurements of turning Powers of Screw Ships.

Fig. 1.

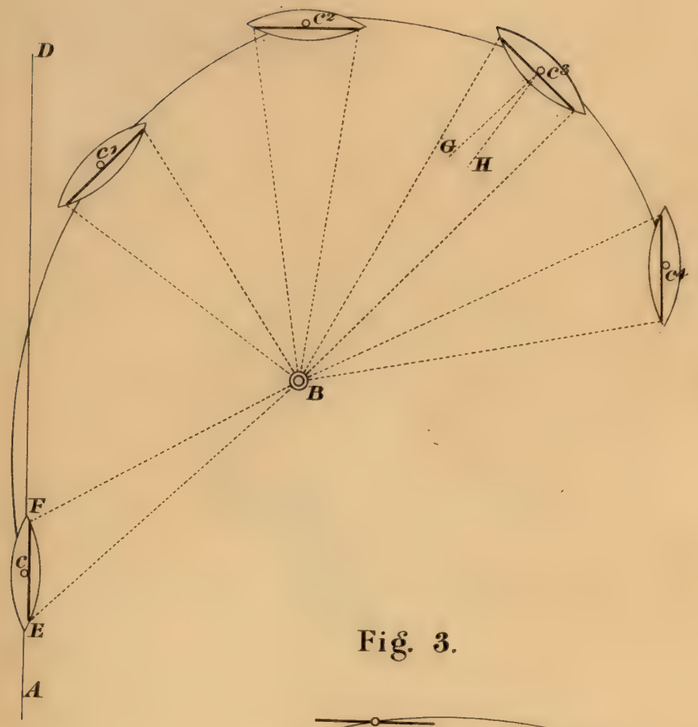


Fig. 2.

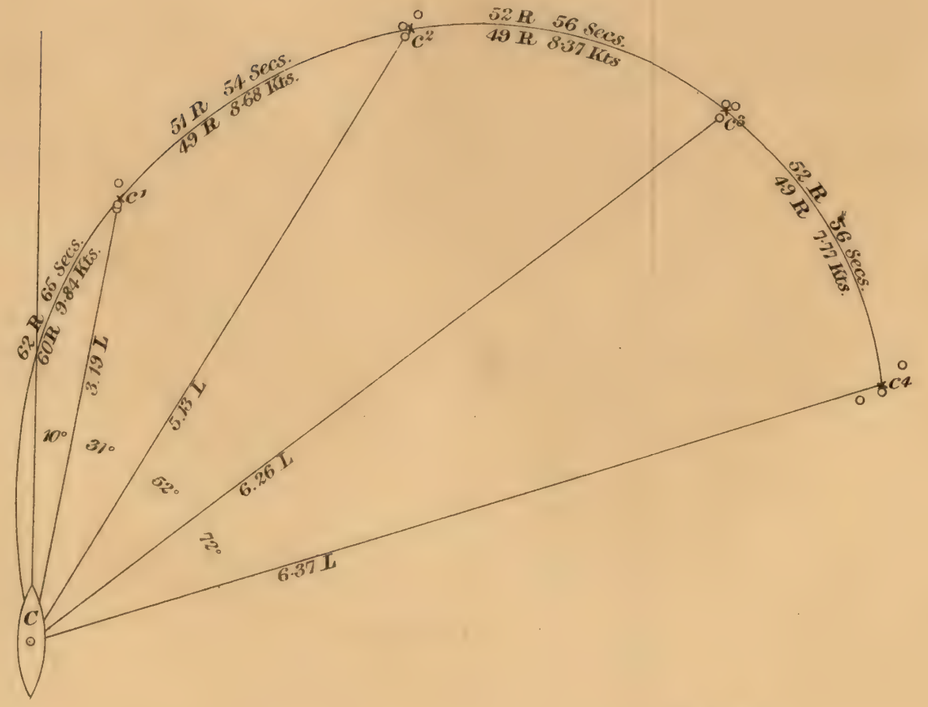


Fig. 4.

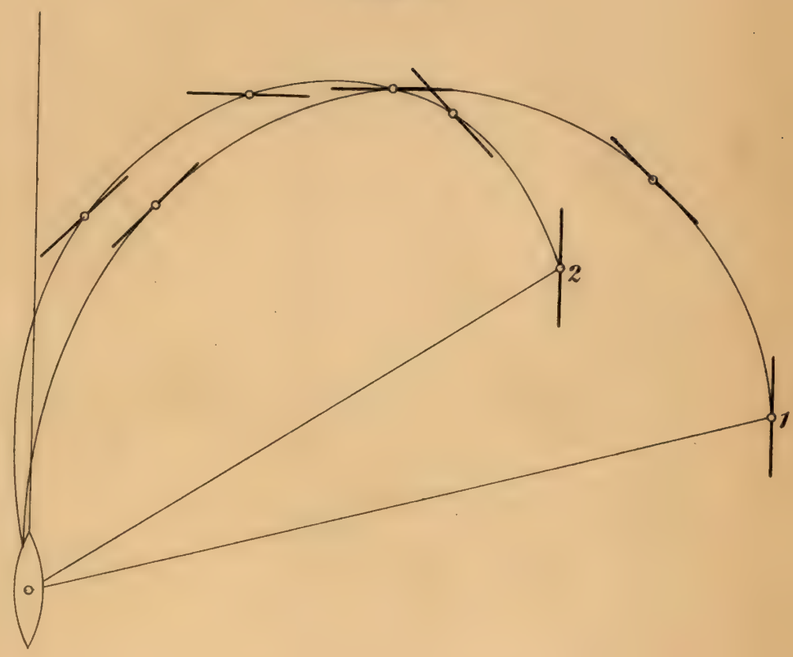


Fig. 3.

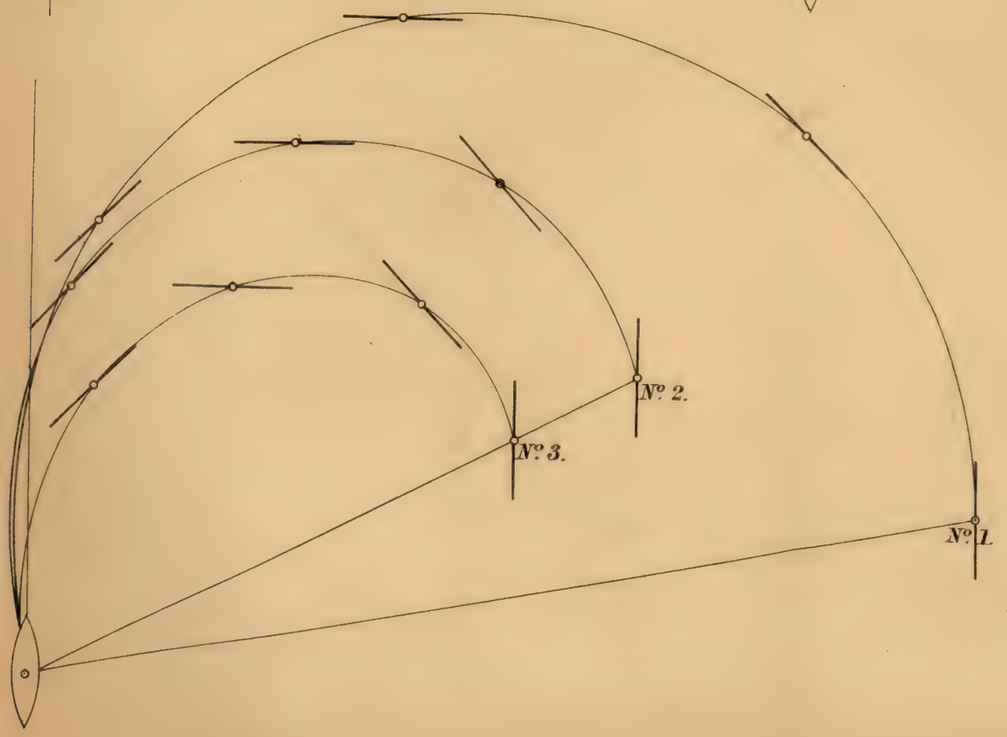
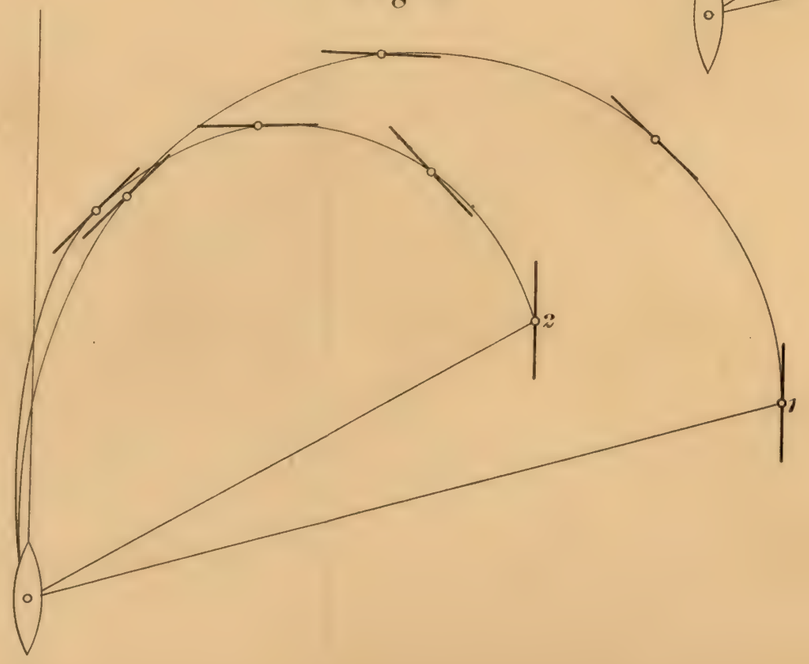


Fig. 5.



To Illustrate Mr. T. C. Read's Paper on the Strength of Bulkheads

FIG. 1.

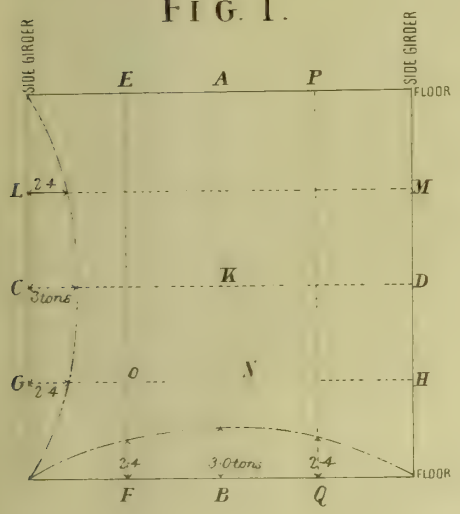


FIG. 3.

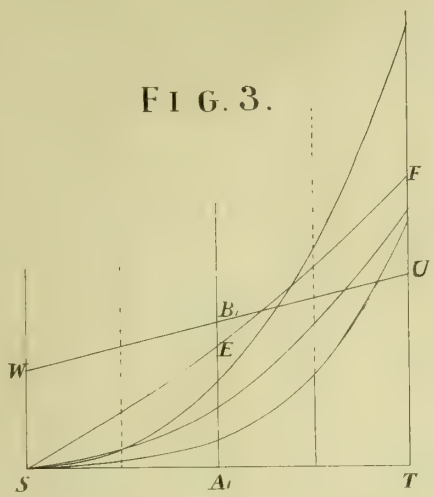


FIG. 2.

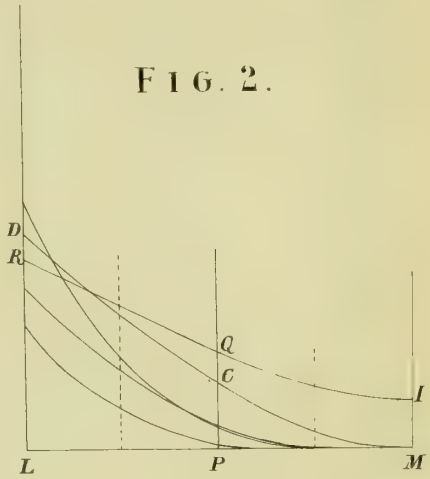


FIG 4.

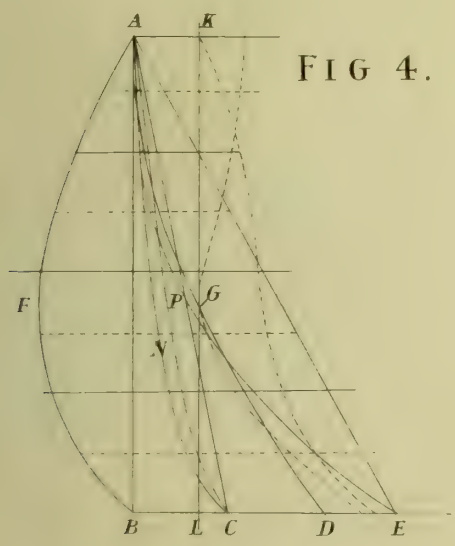


FIG. 5.

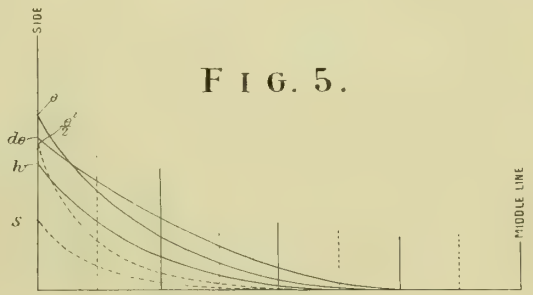


FIG. 8.

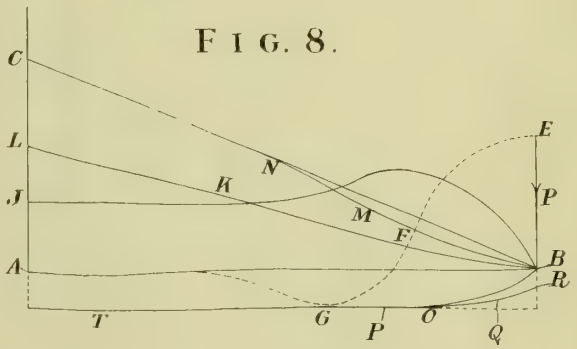


FIG. 7.

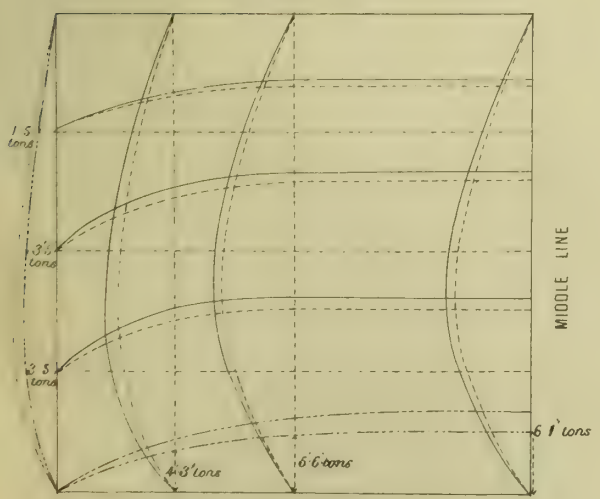
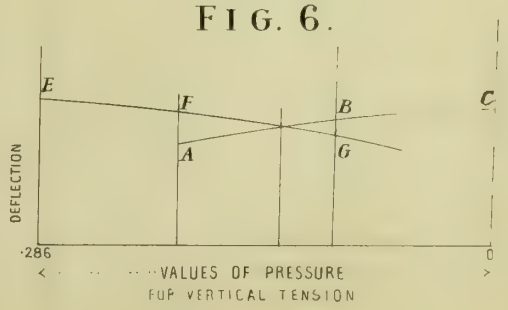


FIG. 6.



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